THE
POPULAR SCIENCE
MONTHLY.

OCTOBER, 1904.

A TRAVELER'S VIEW OF THE BRITISH ASSOCIATION MEETING.

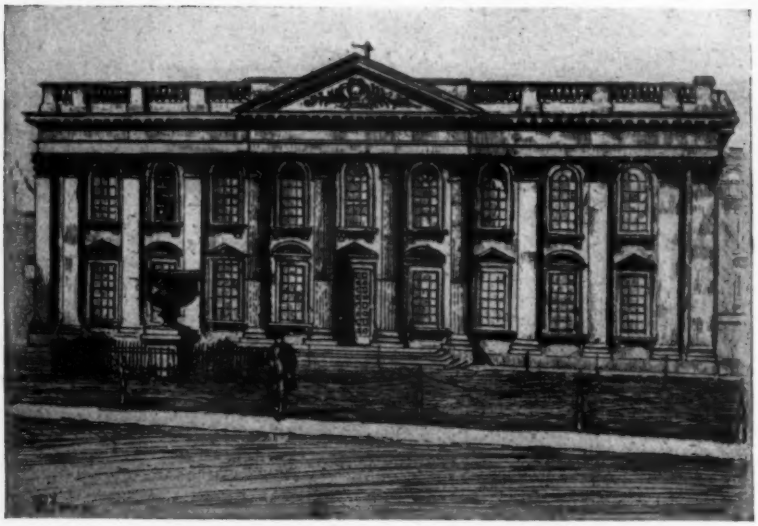
By Dr. HENRY S. PRITCHETT.

PRESIDENT OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

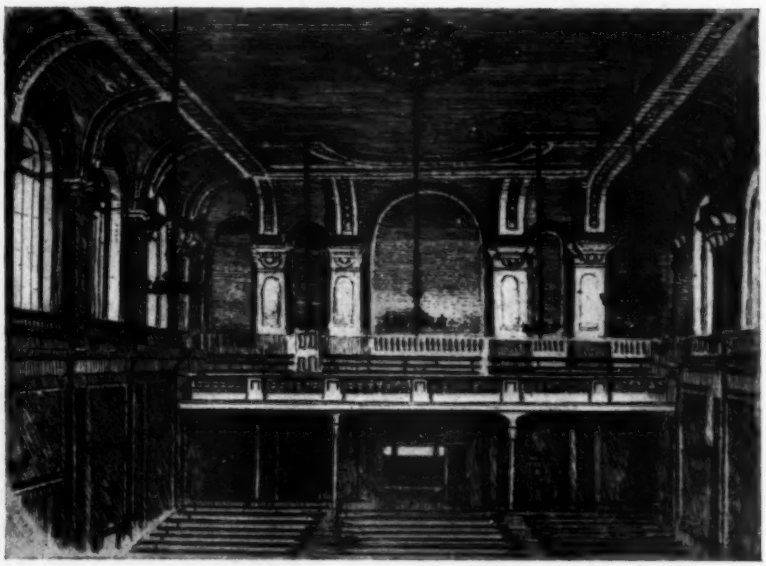
THE meetings of the British Association for the Advancement of Science must always have great interest for Americans; and not alone for scientific men, but for all students of the larger national movements and sources of power in the two great English-speaking countries.

The meeting just closed (August 17–23), held in the old university town of Cambridge, brought together a large number of persons connected with or interested in the science of Great Britain. The registration reached nearly 3,000. In these days when the meetings of the American Association, even under the admirable efforts which have been put forth for some years, have shown a tendency to dwindle, this fact alone is one of interest and of significance to Americans. Two reasons combined to make the attendance at the Cambridge meeting unusually large, first the attractions which naturally belong to this charming old university town, and secondly the presence of the prime minister of Great Britain as president of the association.

To one familiar with the history of our American Association and with the conduct of scientific work in the United States this fact—the presence and active participation of the head of the government—was perhaps the most curious and interesting feature of the meeting. Science and politics have seldom had in our country that close association which one finds in England, Germany and most continental countries. Fancy President Roosevelt taking a week to preside over the meetings of the American Association, to deliver an address and



SENATE HOUSE, WHERE DEGREES WERE CONFERRED.



INTERIOR OF THE GUILD HALL, WHERE THE PRESIDENT'S ADDRESS WAS GIVEN.

The Illustrations are from 'Memorials of Cambridge,' by J. Le Keux and C. H. Cooper and from 'Cambridge' by J. W. Clark.

to participate in its discussions! Or imagine Mr. Speaker Cannon as president of the section of economics and taking a real part in the debates! This lack of touch between scientific men and politicians in our country as compared with the older European countries is due to several causes. The high places in political life are in the older nations mainly in the hands of university men (a process that is going on with us), but more than this the profession of politics in them is not incompatible with the spirit and work of the scholar.

Jefferson more than any other president was a representative of the science of his time. During a part of his first term he was president of the American Philosophical Society and set apart some of the rooms in the executive mansion for the study of fossils, particularly of those of mammoths. It is safe to say that no other president since his day has found the time to give any serious thought to the encouragement of science or of education as a part of national development.

Perhaps the criticisms which Jefferson called down upon himself by his scientific tendencies have not served to encourage other presidents. His geological studies were pointed to, about the time of the Louisiana purchase, with great bitterness by his critics as indicating those radical and godless tendencies which culminated in the act of purchase. There is a poem of William Cullen Bryant on this transaction which is addressed to Jefferson and which begins

> Go wretch, resign the presidential chair;
> Reveal thy secret purpose, foul or fair;

In the course of the poem 'frogs' are significantly made to rhyme with 'Louisianian bogs.' The fact that the poet was but thirteen at the time may be taken as a measure of the sharpness of the criticism which awaits a president who compromises himself by too great intimacy with science. It is easier, if not safer, for a president to look after the post offices and let science take care of herself.

Mr. Balfour himself did not altogether escape this sort of criticism. His address had for a title 'Reflections Suggested by the New Theory of Matter.' The opposition papers were not slow to suggest that the prime minister and practical ruler of a great commercial country could spend his time to better advantage than in discoursing transcendental philosophy to admiring audiences of scientists!

The critics were so far right in terming Mr. Balfour's address philosophical rather than scientific. By disposition and by education Mr. Balfour is a speculative philosopher rather than a man of science, and his address leaned strongly toward that mildly pessimistic attitude of the speculative philosopher, which balances in a nice way this and that conclusion, and goes no whither.

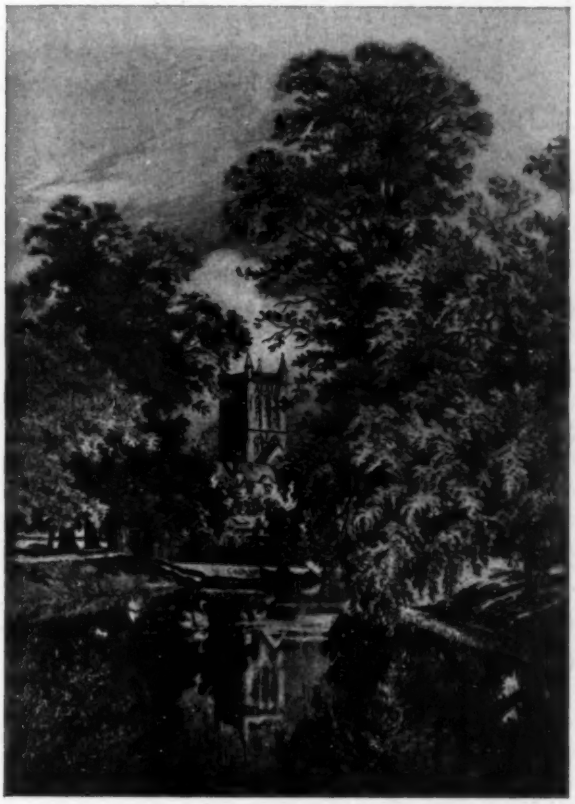
The address sketched a brief comparison between the scientific conception of the physical universe to-day and that of one hundred years ago. It was remarkable in this respect that a man so full of other work, as Mr. Balfour must be, should be able to frame such a statement without committing errors of fact of a serious sort. As an analytic review it had no great value (notwithstanding the one or two ingenious points brought forward) on account of the speaker's lack of expert knowledge in physics and on account of the constant assumption of a position entirely apart from and unlike that of the physical investigator. The address could be called on the whole clever, interesting and suggestive from the philosophical standpoint, and to have

JESUS COLLEGE, FROM THE MEADOWS.

presented such a paper is an evidence of great intellectual alertness and ability on the part of a man whose hands are full of practical business.

The occasion of the presidential address on the evening of the first day was the most interesting event of the meeting and the one which brought together the most interesting audience. Mr. Balfour read his address, explaining that in this he followed precedent, although speaking was easier to him than reading. He spoke with a clear, pleasant voice and in a perfectly natural and easy manner. His delivery throughout was most effective. On the platform beside him sat many of the best known men in British scientific circles, the veteran Lord Kelvin occupying a place to the left of the speaker, and looking like an idealized version of Uncle Joe Cannon.

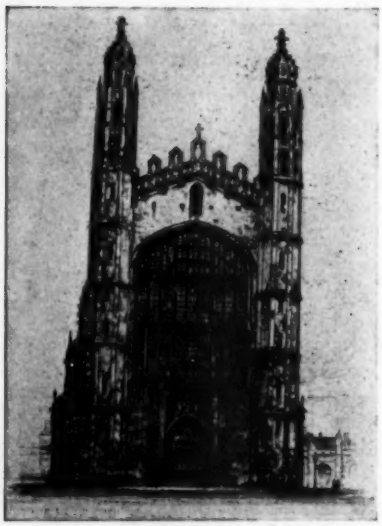
There is one custom always observed on such occasions, which the American learns to recognize after a while as a part of English politeness, but which never ceases to amuse him. I refer to the speeches made in moving a vote of thanks after an address. This custom seems an invariable one; at business boards, at scientific gatherings, at charity meetings the chairman or the speaker is always formally and specifically thanked. The process is as follows: First a distinguished



THE CAM, NEAR TRINITY COLLEGE, WITH THE TOWER OF ST. JOHN'S COLLEGE CHAPEL.

member of the audience (the more distinguished the better) moves a vote of thanks in a speech of greater or less length and full of personal compliment for the speaker; then a second member of the audience of equal distinction, if possible, seconds the resolution in a speech in which he tries to mention all the good points not mentioned by the first. A vote is then taken. It goes without saying that the resolution passes unanimously. The most amusing part of this naive proceeding comes when the original speaker rises to reply to the vote of thanks. The mover of the vote of thanks on the occasion of Mr. Balfour's Cam-

bridge address would naturally have been the chancellor of the University of Cambridge, the Duke of Devonshire, but owing to the political changes of the last eighteen months the duke and the premier could not very comfortably meet on the same platform. The vote was therefore moved by the vice chancellor and seconded by the lord mayor of Cambridge. In his reply Mr. Balfour acquitted himself admirably, showing not only the ability of a polished speaker, but acknowledging the praise of his address in modest and fitting words.

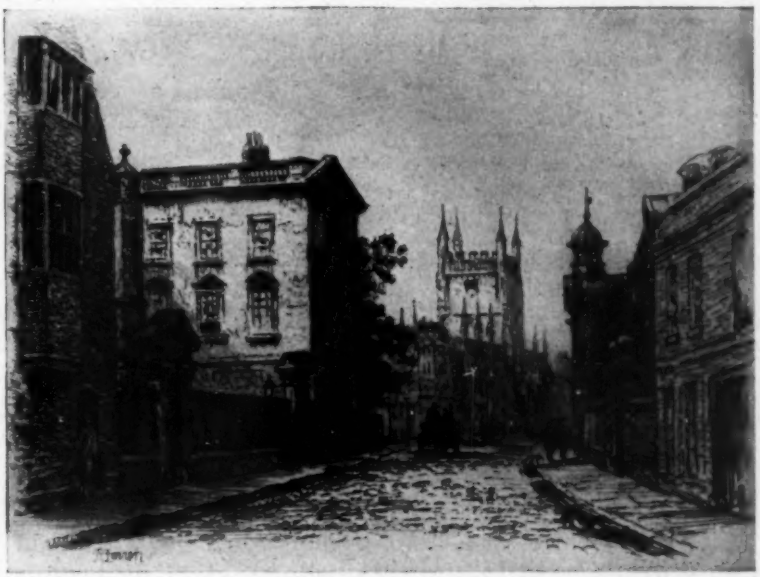


KING'S COLLEGE CHAPEL.

On the following morning visitors in section A (which in the British Association includes both mathematics and physics) had the pleasure of hearing Mr. Balfour in the opposite rôle, as the mover of a vote of thanks to Professor Lamb after the delivery of his address as president of the section; a part which the prime minister filled with equal grace and skill and with as much seriousness as can be made out of such a naive proceeding as this process must be. Mr. Balfour's presidency of the association was no sinecure. He met all its duties most faithfully and carried out the business of the president with admirable tact. He appeared at the meetings of various sections, took part in the discussions of some of them as in the section of economics, and threw himself heartily into the social duties of the meeting. On the evening of the general reception to the members and visitors given in the splendid halls of Trinity College (Mr. Balfour's own college) he is said to have shaken hands with more than three thousand persons, and up to the last belated guest his smile was as cor-

INTERIOR OF KING'S COLLEGE CHAPEL.

dial and his handshake as hearty as at the beginning. For each member, and particularly for all visitors from outside England, he had a kindly word, and a greeting which while most hospitable was never overdone; which carried the pleasure of a friendly welcome without losing at any moment the stamp of good breeding. Except perhaps President McKinley, I have never seen a man in public station who could receive so many persons in a public reception and so successfully make each one feel that he had been given a special welcome. As an American who has seen much of political life remarked, 'A man who can shake hands



PETERHOUSE.

like that would be a successful politician in any country.' Mr. Balfour was in fact an ideal president of the association, democratic, yet dignified. He left on those who saw him for the first time the impression of a man who had not only intellectual power, but also one who combined with this good breeding, good nature and common sense.

A very interesting comparison between the American and the British Association could be found in a study of the sectional addresses and other leading papers of the one as contrasted with the other. In such a comparison the American would find little to minister to national vanity. The presidents of the sections in the British Association are almost always men of assured scientific standing and reputation. Their audiences include many of the best known men of science in England. The addresses are prepared with more care and are generally given in a more interesting and effective manner. The occasion

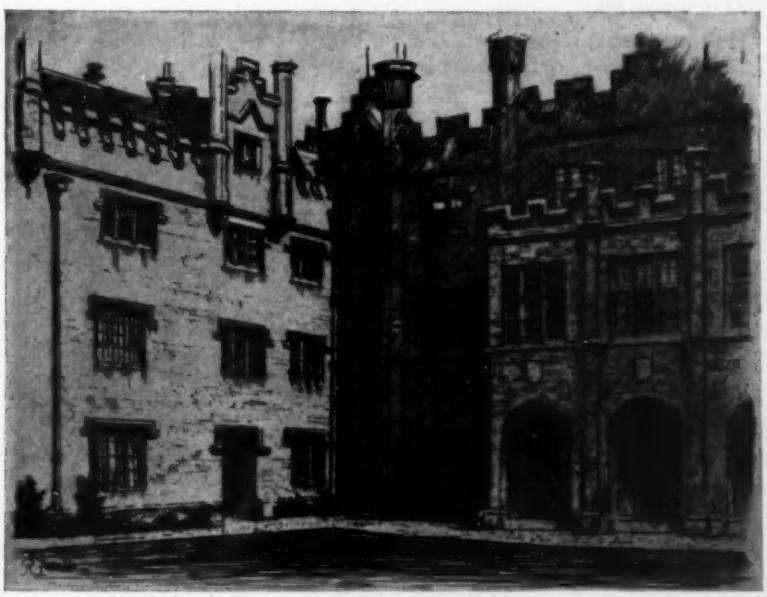
NEVILLE'S COURT, TRINITY COLLEGE.

means more to a speaker than it does with us, at least during the last fifteen years since the tendency to segregate into special scientific societies has been so marked. It would be difficult to bring together in America any such group of mathematicians and physicists as sat with Professor Lamb on the platform on Thursday morning while he read his presidential address before section A. The vote of thanks after the address was moved by Mr. Balfour and seconded by Lord Kelvin.

Nevertheless admitting all this, it is evident to one who listens to papers in both associations that the essential difference in the character of the papers presented at the two meetings lies in the difference in scientific training and habits of scientific work in England and in America; and one can not but be struck with the fact that the scientific training and methods of work in America are far more German than English. While the addresses in American scientific societies lack the philosophic interest and charm which characterize many of those given before the British Association, the authors of these papers are trained to go more directly at their problems, laying bare the difficulties and even the failures of the method or the process, but passing on to some point of vantage. One finds in many English scientific papers a clever use of words and terms; a tendency to philosophize instead of doing the hard work of investigation; a disposition to deal charmingly, sometimes half humorously, with the results and observations costing great labor; and in the end the whole subject left in a sort of agreeable haze in which one seems to have traveled a long distance without going any whither. The method of attack adopted is

somewhat akin to that of the modern military practice, under which frontal attacks are abandoned in favor of a less direct method of assault. One sees in English scientific papers a greater tendency to attack by the flank than in America or Germany; a somewhat readier disposition to be satisfied with a general statement of facts already known rather than the concentration of effort on particular problems which need to be cleared up. All of which simply means that the methods of education and of national life in England have not brought into existence a large army of disciplined students of research such as one finds, for example, in Germany.

The fact that physics and mathematics are still retained in one section in the British Association is not without significance. The necessary connection between the two was many times referred to in the addresses and papers of the section. Notwithstanding this there was more than one reference to the fact that mathematics, as taught in the universities and colleges, is seldom grasped by the student so that it becomes a facile tool in his hands. This is a disappointing fact in our present day teaching on this side the Atlantic no less than in England. Why is it that mathematics, the oldest of the sciences, should lend itself less readily as an instrument of research or of practice than chemistry or physics? Is it because we fail to use the laboratory method in mathematics? or because we are still tied to the methods of the past? or is it due in part to the fact that too little time



FIRST COURT OF SIDNEY SUSSEX COLLEGE.

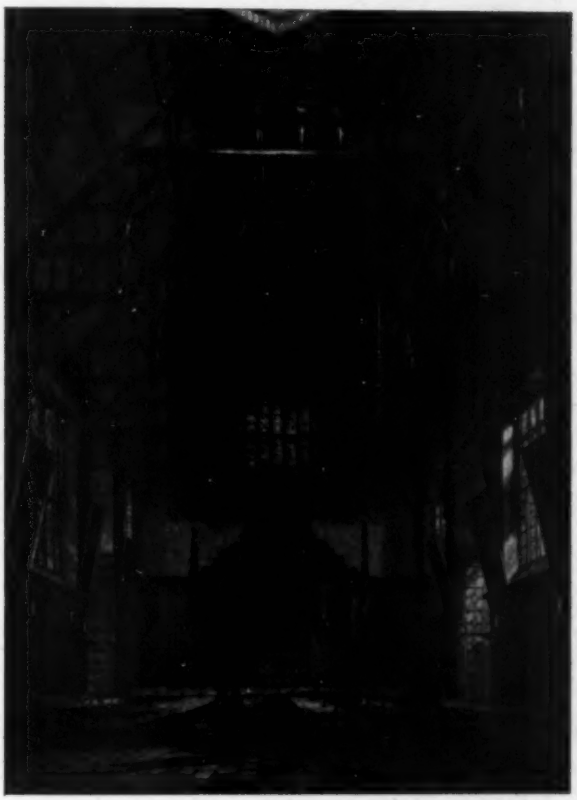
is given to learning elementary mathematical concepts before rushing on to complicated theory and partly perhaps to the fact that those who teach mathematics in the colleges are almost always those who never have occasion to apply it?

The groups of individuals who made up the small army of 2,800 persons registered at the Cambridge meeting were varied. First of all there were the well-known men of science like Lord Kelvin, Lord Rayleigh, Professor Thomson, Professor Forsythe, Professor Dewar and many others. Next in interest came the large group of younger men, probably mostly from Cambridge University, who were to be seen in many of the sections. The great body of associates was made up, as is the case in meetings of the American Association, of the wives, daughters and friends of the members. The women associates, as in America, were in greatest evidence in the social functions, the excursions and in a few of the sections, particularly in that devoted to educational science, the last section organized in the British Association. On Thursday morning before this section Dr. Kölössy, of Budapest, read his paper before an audience of about fifty men and three hundred women.

Considering the present relation of public education in England to the church it was quite natural that the president of this section should be the Lord Bishop of Hereford, himself a teacher of experience. The papers before this section were however devoted to elementary educational questions rather than to problems of educational science, and the discussions which were had, particularly when they touched on such subjects as the education of women or the function of manual training, sounded curiously like those to which we were accustomed in the United States twenty-five or thirty years ago. The questions which occupied the larger part of the time were in large measure local and concerned themselves with details of educational work rather than with fundamental underlying theories of education. This also is perhaps to be expected in a country which has placed its work of education in large measure in the hands of a single section of the protestant church.

To an American visitor the large number of curates of the established church who appeared in the meetings of the association formed a pleasant picture. This presence does not mean any widespread study of science on the part of the clergy, but is rather a natural outgrowth of the association of university men. A large proportion of those who graduate at the universities enter the church, and it is to be expected that in a gathering which brings together distinguished scientific men, as well as those interested in a general way in science, there should be a number of the clergy. Their presence also serves to emphasize another feature of the association which the American is likely to overlook, and this is the fact that the British Association finds its strength

and its influence, in large measure, in the social soil into which it sends its roots. Social influence is one of the most powerful factors in English life and one of the most powerful in politics. The British Association gains much of its prestige from its social and political setting, and in directing social and political power no one interest is so strong as that of the established church.



INTERIOR OF THE HALL, TRINITY COLLEGE.

Looking at this great gathering from the standpoint of an interested outsider, the American who studies it can not but be impressed by its possibilities for usefulness in scientific and in national development. It is a fine thing to bring together the representatives of science, of politics, of religion and to have them meet face to face a large body of men and women drawn from the most intelligent homes in the kingdom. The general effect of all this is somewhat neutralized by the social machinery through which it works, but allowing for all this it still seems evident that such a gathering is a source of great intellectual stimulus both to the scientific men and to the public. It is no

small thing to present a paper in a section where so many famous men sat as could be seen in section A at almost any time of its sitting. And it is no small credit to British men of science that this great gathering is preserved from year to year in undiminished enthusiasm.

Is it possible to make of the American Association such a gathering, or rather to restore to it its representative character? May we expect to gather to it the well-known men of science, the ambitious students and the scientific public?

Many things make against such a result in America which are here favorable to it. The small distances to be traveled in Great Britain make it easy and cheap for any member to come to the meetings. Again the British Association flourishes and gains its influence in the midst of a social régime entirely differnt from that which holds in our country. But in addition to all this are the differences in scientific training which prompt the American investigator, whether in pure or applied science, to prefer the society of his fellow experts to any gathering of a general character however attractive it may be by reason of the presence of scientific, political or social celebrities. That which makes the success of the British Association possible is at once the weakness as well as the strength of British science, and the influences which make these meetings what they are are intimately connected with the educational and social conditions which exist in England.

But if there is anything which would bring back once more to the American Association its old-time prestige and its old-time influence, it would be some such devotion to the cause, which the association represents, as has been shown by many of the leading men of science in England. Of all this group perhaps no other one has done so much as Lord Kelvin. For half a century this splendid old man (loved no less for his sweetness than honored for his genius) has been a prominent figure at these annual gatherings. Year after year he has not only presented his own brilliant contributions and taken his part in the discussions, but there has scarcely been presented in all these years, in the subjects in which he stands preeminent, a worthy paper by a struggling younger man whom this Nestor of British science has not encouraged by his words of praise or of friendly advice given in the most kindly and helpful spirit. The example and influence of such a man are beyond praise, and however we may criticize English science, and English methods of education, we may well hope to learn of her many things so long as she produces men like Kelvin. If anything can make of the American Association a new center of inspiration for younger men, a fresh source of popular education, a means of closer union of our scientific interests, it will come only as the result of some such unselfish service as Kelvin's on the part of the best known men of science in America.

REFLECTIONS SUGGESTED BY THE NEW THEORY OF MATTER.

BY THE RIGHT HONORABLE ARTHUR JAMES BALFOUR,
PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE meetings of this great society have for the most part been held in crowded centers of population, where our surroundings never permit us to forget, were such forgetfulness in any case possible, how close is the tie that binds modern science to modern industry, the abstract researches of the student to the labors of the inventor and the mechanic. This, no doubt, is as it should be. The interdependence of theory and practise can not be ignored without inflicting injury on both; and he is but a poor friend to either who undervalues their mutual cooperation.

Yet, after all, since the British Association exists for the advancement of science, it is well that now and again we should choose our place of gathering in some spot where science rather than its applications, knowledge, not utility, are the ends to which research is primarily directed.

If this be so, surely no happier selection could have been made than the quiet courts of this ancient university. For here, if anywhere, we tread the classic ground of physical discovery. Here, if anywhere, those who hold that physics is the true *scientia scientiarum*, the root of all the sciences which deal with inanimate nature, should feel themselves at home. For, unless I am led astray by too partial an affection for my own university, there is nowhere to be found, in any corner of the world, a spot with which have been connected, either by their training in youth, or by the labors of their maturer years, so many men eminent as the originators of new and fruitful physical conceptions. I say nothing of Bacon, the eloquent prophet of a new era; nor of Darwin, the Copernicus of biology; for my subject to-day is not the contributions of Cambridge to the general growth of scientific knowledge. I am concerned rather with the illustrious line of physicists who have learned or taught within a few hundred yards of this building;—a line stretching from Newton in the seventeenth century, through Cavendish in the eighteenth, through Young, Stokes, Maxwell, in the nineteenth, through Kelvin, who embodies an epoch in himself, down to Rayleigh, Larmor, J. J. Thomson, and the scientific school centered in the Cavendish laboratory, whose physical speculations bid fair to render the

closing years of the old century and the opening years of the new as notable as the greatest which have preceded them.

Now what is the task which these men, and their illustrious fellow-laborers out of all lands, have set themselves to accomplish? To what end led these 'new and fruitful physical conceptions' to which I have just referred? It is often described as the discovery of the 'laws connecting phenomena.' But this is certainly a misleading, and in my opinion a very inadequate account of the subject. To begin with, it is not only inconvenient, but confusing, to describe as 'phenomena' things which do not appear, which never have appeared, and which never can appear, to beings so poorly provided as ourselves with the apparatus of sense perception. But apart from this, which is a linguistic error too deeply rooted to be easily exterminated, is it not most inaccurate in substance to say that a knowledge of nature's laws is all we seek when investigating nature? The physicist looks for something more than what by any stretch of language can be described as 'coexistences' and 'sequences' between so-called 'phenomena.' He seeks for something deeper than the laws connecting possible objects of experience. His object is physical reality; a reality which may or may not be capable of direct perception; a reality which is in any case independent of it; a reality which constitutes the permanent mechanism of that physical universe with which our immediate empirical connection is so slight and so deceptive. That such a reality exists, though philosophers have doubted, is the unalterable faith of science; and were that faith *per impossible* to perish under the assaults of critical speculation, science, as men of science usually conceive it, would perish likewise.

If this be so, if one of the tasks of science, and more particularly of physics, is to frame a conception of the physical universe in its inner reality, then any attempt to compare the different modes in which, at different epochs of scientific development, this intellectual picture has been drawn, can not fail to suggest questions of the deepest interest. True, I am precluded from dealing with such of these questions as are purely philosophical by the character of this occasion; and with such of them as are purely scientific by my own incompetence. But some there may be sufficiently near the dividing line to induce the specialists who rule by right on either side of it, to view with forgiving eyes any trespasses into their legitimate domain which I may be tempted, during the next few minutes, to commit.

Let me then endeavor to compare the outlines of two such pictures, of which the first may be taken to represent the views prevalent towards the end of the eighteenth century; a little more than a hundred years from the publication of Newton's 'Principia,' and, roughly speaking, about midway between that epoch-making date and the present mo-

ment. I suppose that if at that period the average man of science had been asked to sketch his general conception of the physical universe, he would probably have said that it essentially consisted of various sorts of ponderable matter, scattered in different combinations through space, exhibiting most varied aspects under the influence of chemical affinity and temperature, but through every metamorphosis obedient to the laws of motion, always retaining its mass unchanged, and exercising at all distances a force of attraction on other material masses, according to a simple law. To this ponderable matter he would (in spite of Rumford) have probably added the so-called 'imponderable' heat, then often ranked among the elements; together with the two 'electrical fluids,' and the corpuscular emanations supposed to constitute light.

In the universe as thus conceived, the most important forms of action between its constituents was action at a distance; the principle of the conservation of energy was, in any general form, undreamed of; electricity and magnetism, though already the subjects of important investigation, played no great part in the whole of things; nor was a diffused ether required to complete the machinery of the universe.

Within a few months, however, of the date assigned for these deliverances of our hypothetical physicist, came an addition to this general conception of the world, destined profoundly to modify it. About a hundred years ago Young opened, or reopened, the great controversy which finally established the undulatory theory of light, and with it a belief in an interstellar medium by which undulations could be conveyed. But this discovery involved much more than the substitution of a theory of light which was consistent with the facts, for one which was not; since here was the first authentic introduction* into the scientific world picture of a new and prodigious constituent—a constituent which has altered, and is still altering, the whole balance (so to speak) of the composition. Unending space, thinly strewn with suns and satellites, made or in the making, supplied sufficient material for the mechanism of the heavens as conceived by Laplace. Unending space filled with a continuous medium was a very different affair, and gave promise of strange developments. It could not be supposed that the ether, if its reality were once admitted, existed only to convey through interstellar regions the vibrations which happen to stimulate the optic nerve of man. Invented originally to fulfil this function, to this it could never be confined. And accordingly, as every one now knows, things which, from the point of view of sense perception, are as distinct as light and radiant heat; and things to which sense per-

* The hypothesis of an ether was, of course, not new. But before Young and Fresnel it can not be said to have been established.

ception makes no response, like the electric waves of wireless telegraphy,* intrinsically differ, not in kind but in magnitude alone.

This, however, is not all, nor nearly all. If we jump over the century which separates 1804 from 1904, and attempt to give in outline the world picture as it now presents itself to some leaders of contemporary speculation, we shall find that in the interval it has been modified, not merely by such far-reaching discoveries as the atomic and molecular composition of ordinary matter, the kinetic theory of gases, and the laws of the conservation and dissipation of energy; but by the more and more important part which electricity and the ether occupy in any representation of ultimate physical reality.

Electricity was no more to the natural philosophers in the year 1700 than the hidden cause of an insignificant phenomenon.† It was known, and had long been known, that such things as amber and glass could be made to attract light objects brought into their neighborhood; yet it was about fifty years before the effects of electricity were perceived in the thunderstorm. It was about one hundred years before it was detected in the form of a current. It was about one hundred and twenty years before it was connected with magnetism; about one hundred and seventy years before it was connected with light and ethereal radiation.

But to-day there are those who regard gross matter, the matter of every-day experience, as the mere appearance of which electricity is the physical basis: who think that the elementary atom of the chemist, itself far beyond the limits of direct perception, is but a connected system of monads or subatoms which are not electrified matter, but are electricity itself; that these systems differ in the number of monads which they contain, in their arrangement, and in their motion relative to each other and to the ether; that on these differences, and on these differences alone, depend the various qualities of what have hitherto been regarded as indivisible and elementary atoms; and that while in most cases these atomic systems may maintain their equilibrium for periods which, compared with such astronomical processes as the cooling of a sun, may seem almost eternal, they are not less obedient to the law of change than the everlasting heavens themselves.

But if gross matter be a grouping of atoms, and if atoms be systems of electrical monads, what are these electrical monads? It may be that, as Professor Larmor has suggested, they are but a modification of the universal ether, a modification roughly comparable to a knot in a medium which is inextensible, incompressible and continu-

* First known through the theoretical work of Maxwell and the experiments of Hertz.

† The modern history of electricity begins with Gilbert, but I have throughout confined my observations to the post-Newtonian period.

ous. But whether this final unification be accepted or not, it is certain that these monads can not be considered apart from the ether. It is on their interaction with the ether that their qualities depend—and without the ether an electric theory of matter is impossible.

Surely we have here a very extraordinary revolution. Two centuries ago electricity seemed but a scientific toy. It is now thought by many to constitute the reality of which matter is but the sensible expression. It is but a century ago that the title of an ether to a place among the constituents of the universe was authentically established. It seems possible now that it may be the stuff out of which that universe is wholly built. Nor are the collateral inferences associated with this view of the physical world less surprising. It used, for example, to be thought that mass was an original property of matter: neither capable of explanation nor requiring it; in its nature essentially unchangeable, suffering neither augmentation nor diminution under the stress of any forces to which it could be subjected; unalterably attached to, or identified with, each material fragment, howsoever much that fragment might vary in its appearance, its bulk, its chemical, or its physical condition.

But if the new theories be accepted these views must be revised. Mass is not only explicable, it is actually explained. So far from being an attribute of matter considered in itself, it is due, as I have said, to the relation between the electrical monads of which matter is composed and the ether in which they are bathed. So far from being unchangeable, it changes, when moving at very high speeds, with every change in its velocity.

Perhaps, however, the most impressive alteration in our picture of the universe required by these new theories is to be sought in a different direction. We have all, I suppose, been interested in the generally accepted views as to the origin and development of suns with their dependent planetary systems; and the gradual dissipation of the energy which during this process of concentration has largely taken the form of light and radiant heat. Follow out the theory to its obvious conclusions, and it becomes plain that the stars now visibly incandescent are those in mid-journey between the nebulæ from which they sprang and the frozen darkness to which they are predestined. What, then, are we to think of the invisible multitude of the heavenly bodies in which this process has been already completed? According to the ordinary view, we should suppose them to be in a state where all possibilities of internal movement were exhausted. At the temperature of interstellar space their constituent elements would be solid and inert; chemical action and molecular movement would be alike impossible, and their exhausted energy could obtain no replenishment unless they were suddenly rejuvenated by some celestial collision, or traveled into other regions warmed by newer suns.

This view must, however, be profoundly modified if we accept the electric theory of matter. We can then no longer hold that if the internal energy of a sun were as far as possible converted into heat either by its contraction under the stress of gravitation or by chemical reactions between its elements or by any other interatomic force; and that were the heat so generated to be dissipated, as in time it must be, through infinite space, its whole energy would be exhausted. On the contrary, the amount thus lost would be absolutely insignificant compared with what remained stored up within the separate atoms. The system in its corporate capacity would become bankrupt—the wealth of its individual constituents would be scarcely diminished. They would lie side by side, without movement, without chemical affinity; yet each one, howsoever inert in its external relations, the theater of violent motions, and of powerful internal forces.

Or put the same thought in another form: when the sudden appearance of some new star in the telescopic field gives notice to the astronomer that he, and, perhaps, in the whole universe, he alone, is witnessing the conflagration of a world; the tremendous forces by which this far-off tragedy is being accomplished must surely move his awe. Yet not only would the members of each separate atomic system pursue their relative course unchanged, while the atoms themselves were thus riven violently apart in flaming vapor, but the forces by which such a world is shattered are really negligible compared with those by which each atom of it is held together.

In common, therefore, with all other living things we seem to be practically concerned chiefly with the feebler forces of nature, and with energy in its least powerful manifestations. Chemical affinity and cohesion are on this theory no more than the slight residual effects of the internal electrical forces which keep the atom in being. Gravitation, though it be the shaping force which concentrates nebulæ into organized systems of suns and satellites, is trifling compared with the attractions and repulsions with which we are familiar between electrically charged bodies; while these again sink into insignificance beside the attractions and repulsions between the electric monads themselves. The irregular molecular movements which constitute heat, on which the very possibility of organic life seems absolutely to hang, and in whose transformations applied science is at present so largely concerned, can not rival the kinetic energy stored within the molecules themselves. This prodigious mechanism seems outside the range of our immediate interests. We live, so to speak, merely on its fringe. It has for us no promise of utilitarian value. It will not drive our mills; we can not harness it to our trains. Yet not less on that account does it stir the intellectual imagination. The starry heavens have, from time immemorial, moved the worship or the wonder of man-

kind. But if the dust beneath our feet be indeed compounded of innumerable systems, whose elements are ever in the most rapid motion, yet retain through uncounted ages their equilibrium unshaken, we can hardly deny that the marvels we directly see are not more worthy of admiration than those which recent discoveries have enabled us dimly to surmise.

Now whether the main outlines of the world-picture which I have just imperfectly presented to you be destined to survive, or whether in their turn they are to be obliterated by some new drawing on the scientific palimpsest, all will, I think, admit that so bold an attempt to unify physical nature excites feelings of the most acute intellectual gratification. The satisfaction it gives is almost esthetic in its intensity and quality. We feel the same sort of pleasurable shock as when from the crest of some melancholy pass we first see far below us the sudden glories of plain, river and mountain. Whether this vehement sentiment in favor of a simple universe has any theoretical justification, I will not venture to pronounce. There is no *a priori* reason that I know of for expecting that the material world should be a modification of a single medium, rather than a composite structure built out of sixty or seventy elementary substances, eternal and eternally different. Why, then, should we feel content with the first hypothesis and not with the second? Yet so it is. Men of science have always been restive under the multiplication of entities. They have eagerly noted any sign that the chemical atom was composite, and that the different chemical elements had a common origin. Nor for my part do I think such instincts should be ignored. John Mill, if I rightly remember, was contemptuous of those who saw any difficulty in accepting the doctrine of 'action at a distance.' So far as observation and experiment can tell us, bodies *do* actually influence each other at a distance; and why should they not? Why seek to go behind experience in obedience to some *a priori* sentiment for which no argument can be adduced? So reasoned Mill, and to his reasoning I have no reply. Nevertheless, we can not forget that it is to Faraday's obstinate disbelief in 'action at a distance,' that we owe some of the crucial discoveries on which both our electric industries and the electric theory of matter are ultimately founded. While at this very moment physicists, however baffled in the quest for an explanation of gravity, refuse altogether to content themselves with the belief, so satisfying to Mill, that it is a simple and inexplicable property of masses acting on each other across space.

These obscure intimations about the nature of reality deserve, I think, more attention than has yet been given to them. That they exist is certain; that they modify the indifferent impartiality of pure empiricism can hardly be denied. The common notion that he who

would search out the secrets of nature must humbly wait on experience, obedient to its slightest hint, is but partly true. This may be his ordinary attitude; but now and again it happens that observation and experiment are not treated as guides to be meekly followed, but as witnesses to be broken down in cross-examination. Their plain message is disbelieved, and the investigating judge does not pause until a confession in harmony with his preconceived ideas has, if possible, been wrung from their reluctant evidence.

This proceeding needs neither explanation nor defense in those cases where there is an apparent contradiction between the utterances of experience in different connections. Such contradictions must of course be reconciled, and science can not rest until the reconciliation is effected. The difficulty really arises when experience apparently says one thing and scientific instinct persists in saying another. Two such cases I have already mentioned; others will easily be found by those who care to seek. What is the origin of this instinct, and what its value; whether it be a mere prejudice to be brushed aside, or a clue which no wise man would disdain to follow, I can not now discuss. For other questions there are, not new, yet raised in an acute form by these most modern views of matter, on which I would ask your indulgent attention for yet a few moments.

That these new views diverge violently from those suggested by ordinary observation is plain enough. No scientific education is likely to make us, in our unreflective moments, regard the solid earth on which we stand, or the organized bodies with which our terrestrial fate is so intimately bound up, as consisting wholly of electric monads very sparsely scattered through the spaces which these fragments of matter are, by a violent metaphor, described as 'occupying.' Not less plain is it that an almost equal divergence is to be found between these new theories and that modification of the common-sense view of matter with which science has in the main been content to work.

What was this modification of common sense? It is roughly indicated by an old philosophic distinction drawn between what were called the 'primary' and the 'secondary' qualities of matter. The primary qualities, such as shape and mass, were supposed to possess an existence quite independent of the observer; and so far the theory agreed with common sense. The secondary qualities, on the other hand, such as warmth and color, were thought to have no such independent existence; being, indeed, no more than the resultants due to the action of the primary qualities on our organs of sense perception;—and here, no doubt, common sense and theory parted company.

You need not fear that I am going to drag you into the controversies with which this theory is historically connected. They have left abiding traces on more than one system of philosophy. They are not

yet solved. In the course of them the very possibility of an independent physical universe has seemed to melt away under the solvent powers of critical analysis. But with all this I am not now concerned. I do not propose to ask what proof we have that an external world exists, or how, if it does exist, we are able to obtain cognizance of it. These may be questions very proper to be asked by philosophy; but they are not proper questions to be asked by science. For, logically, they are antecedent to science, and we must reject the sceptical answers to both of them before physical science becomes possible at all. My present purpose requires me to do no more than observe that, be this theory of the primary and secondary qualities of matter good or bad, it is the one on which science has in the main proceeded. It was with matter thus conceived that Newton experimented. To it he applied his laws of motion; of it he predicated universal gravitation. Nor was the case greatly altered when science became as much preoccupied with the movements of molecules as it was with those of planets. For molecules and atoms, whatever else might be said of them, were at least pieces of matter, and, like other pieces of matter, possessed those 'primary' qualities supposed to be characteristic of all matter, whether found in large masses or in small.

But the electric theory which we have been considering carries us into a new region altogether. It does not confine itself to accounting for the secondary qualities by the primary, or the behavior of matter in bulk by the behavior of matter in atoms; it analyzes matter, whether molar or molecular, into something which is not matter at all. The atom is now no more than the relatively vast theater of operations in which minute monads perform their orderly evolutions; while the monads themselves are not regarded as units of matter, but as units of electricity; so that matter is not merely explained, but is explained away.

Now the point to which I desire to call attention is not to be sought in the great divergence between matter as thus conceived by the physicist and matter as the ordinary man supposes himself to know it, between matter as it is perceived and matter as it really is, but to the fact that the first of these two quite inconsistent views is wholly based on the second.

This is surely something of a paradox. We claim to found all our scientific opinions on experience; and the experience on which we found our theories of the physical universe is our *sense perception* of that universe. That *is* experience; and in this region of belief there is no other. Yet the conclusions which thus profess to be entirely founded upon experience are to all appearance fundamentally opposed to it; our knowledge of reality is based upon illusion; and the very conceptions we use in describing it to others, or in thinking of it our-

selves, are abstracted from anthropomorphic fancies, which science forbids us to believe and nature compels us to employ.

We here touch the fringe of a series of problems with which inductive logic ought to deal; but which that most unsatisfactory branch of philosophy has systematically ignored. This is no fault of men of science. They are occupied in the task of making discoveries, not in that of analyzing the fundamental presuppositions which the very possibility of making discoveries implies. Neither is it the fault of transcendental metaphysicians. Their speculations flourish on a different level of thought: their interest in a philosophy of nature is lukewarm; and howsoever the questions in which they are chiefly concerned be answered, it is by no means certain that the answers will leave the humbler difficulties at which I have hinted either nearer to, or further from, a solution. But though men of science and idealists stand acquitted, the same can hardly be said of empirical philosophers. So far from solving the problem, they seem scarcely to have understood that there was a problem to be solved. Led astray by a misconception to which I have already referred; believing that science was concerned only with (so-called) 'phenomena,' that it had done all that it could be asked to do if it accounted for the sequence of our individual sensations, that it was concerned only with the 'laws of nature,' and not with the inner character of physical reality; disbelieving, indeed, that any such physical reality does in truth exist;—it has never felt called upon seriously to consider what are the actual methods by which science attains its results, and how those methods are to be justified. If any one, for example, will take up Mill's logic, with its 'sequences and coexistences between phenomena,' its 'method of difference,' its 'method of agreement,' and the rest: if he will then compare the actual doctrines of science with this version of the mode in which those doctrines have been arrived at, he will soon be convinced of the exceedingly thin intellectual fare which has so often been served out to us under the imposing title of inductive theory.

There is an added emphasis given to these reflections by a train of thought which has long interested me, though I acknowledge that it never seems to have interested any one else. Observe, then, that in order of logic sense perceptions supply the premises from which we draw all our knowledge of the physical world. It is they which tell us there *is* a physical world; it is on their authority that we learn its character. But in order of causation they are effects due (in part) to the constitution of our organs of sense. What we see depends not merely on what there is to be seen, but on our eyes. What we hear depends not merely on what there is to hear, but on our ears. Now, eyes and ears, and all the mechanism of perception, have, as we know, been evolved in us and our brute progenitors by the slow operation of

natural selection. And what is true of sense perception is of course also true of the intellectual powers which enable us to erect upon the frail and narrow platform which sense perception provides, the proud fabric of the sciences.

Now natural selection only works through utility. It encourages aptitudes useful to their possessor or his species in the struggle for existence, and, for a similar reason, it is apt to discourage useless aptitudes, however interesting they may be from other points of view, because, being useless, they are probably burdensome.

But it is certain that our powers of sense perception and of calculation were fully developed ages before they were effectively employed in searching out the secrets of physical reality—for our discoveries in this field are triumphs but of yesterday. The blind forces of natural selection which so admirably simulate design when they are providing for a present need, possess no power of prevision; and could never, except by accident, have endowed mankind, while in the making, with a physiological or mental outfit adapted to the higher physical investigations. So far as natural science can tell us, every quality of sense or intellect which does not help us to fight, to eat and to bring up children, is but a by-product of the qualities which do. Our organs of sense perception were not given us for purposes of research; nor was it to aid us in meting out the heavens or dividing the atom that our powers of calculation and analysis were evolved from the rudimentary instincts of the animal.

It is presumably due to these circumstances that the beliefs of all mankind about the material surroundings in which it dwells are not only imperfect but fundamentally wrong. It may seem singular that down to, say, five years ago, our race has, without exception, lived and died in a world of illusions; and that its illusions, or those with which we are here alone concerned, have not been about things remote or abstract, things transcendental or divine, but about what men see and handle, about those 'plain matters of fact' among which common sense daily moves with its most confident step and most self-satisfied smile. Presumably, however, this is either because too direct a vision of physical reality was a hindrance, not a help, in the struggle for existence, because falsehood was more useful than truth—or else because with so imperfect a material as living tissue no better results could be attained. But if this conclusion be accepted, its consequences extend to other organs of knowledge besides those of perception. Not merely the senses, but the intellect must be judged by it; and it is hard to see why evolution, which has so lamentably failed to produce trustworthy instruments for obtaining the raw material of experience, should be credited with a larger measure of success in its provision of the physiological arrangements which condition reason in its endeavors to turn experience to account.

Considerations like these, unless I have compressed them beyond the limits of intelligibility, do undoubtedly suggest a certain inevitable incoherence in any general scheme of thought which is built out of materials provided by natural science alone. Extend the boundaries of knowledge as you may; draw how you will the picture of the universe; reduce its infinite variety to the modes of a single space-filling ether; retrace its history to the birth of existing atoms; show how under the pressure of gravitation they became concentrated into nebulæ, into suns, and all the host of heaven; how, at least in one small planet, they combined to form organic compounds; how organic compounds became living things; how living things, developing along many different lines, gave birth at last to one superior race; how from this race arose, after many ages, a learned handful, who looked round on the world which thus blindly brought them into being, and judged it, and knew it for what it was: perform (I say) all this, and though you may indeed have attained to science, in nowise will you have attained to a self-sufficing system of beliefs. One thing at least will remain, of which this long-drawn sequence of causes and effects gives no satisfying explanation; and that is knowledge itself. Natural science must ever regard knowledge as the product of irrational conditions, for in the last resort it knows no others. It must always regard knowledge as rational, or else science itself disappears. In addition, therefore, to the difficulty of extracting from experience beliefs which experience contradicts, we are confronted with the difficulty of harmonizing the pedigree of our beliefs with their title to authority. The more successful we are in explaining their origin, the more doubt we cast on their validity. The more imposing seems the scheme of what we know, the more difficult it is to discover by what ultimate criteria we claim to know it.

Here, however, we touch the frontier beyond which physical science possesses no jurisdiction. If the obscure and difficult region which lies beyond is to be surveyed and made accessible, philosophy, not science, must undertake the task. It is no business of this society. We meet here to promote the cause of knowledge in one of its great divisions; we shall not help it by confusing the limits which usefully separate one division from another. It may perhaps be thought that I have disregarded my own precept; that I have wilfully overstepped the ample bounds within which the searchers into nature carry on their labors. If it be so, I can only beg your forgiveness. My first desire has been to rouse in those, who, like myself, are no specialists in physics, the same absorbing interest which I feel in what is surely the most far-reaching speculation about the physical universe which has ever claimed experimental support; and if in so doing I have been tempted to hint my own personal opinion, that as natural science grows it leans more, not less, upon an idealistic interpretation of the universe, even those who least agree may perhaps be prepared to pardon.

THE MATHEMATICAL PHYSICS OF THE NINETEENTH CENTURY.

By Professor HORACE LAMB, LL.D., D.Sc., F.R.S.,
PRESIDENT OF THE MATHEMATICAL AND PHYSICAL SECTION OF THE BRITISH ASSOCIATION.

THE losses sustained by mathematical science in the past twelve months have perhaps not been so numerous as in some years, but they include at least one name of world-wide import. Those of us who were students of mathematics thirty or forty years ago will recall the delight which we felt in reading the geometrical treatises of George Salmon, and the brilliant contrast which they exhibited with most of the current text-books of that time. It was from him that many of us first learned that a great mathematical theory does not consist of a series of detached propositions carefully labeled and arranged like specimens on the shelves of a museum, but that it forms an organic whole, instinct with life, and with unlimited possibilities of future development. As systematic expositions of the actual state of the science, in which enthusiasm for what is new is tempered by a due respect for what is old, and in which new and old are brought into harmonious relation with each other, these treatises stand almost unrivaled. Whether in the originals, or in the guise of translations, they are accounted as classics in every university of the world. So far as British universities are concerned, they have formed the starting point of a whole series of works conceived in a similar spirit, though naturally not always crowned by the same success. The necessity for this kind of work grows, indeed, continually; the modern fragmentary fashion of original publication and the numerous channels through which it takes place make it difficult for any one to become initiated into a new scientific theory unless he takes it up at the very beginning and follows it diligently throughout its course, backwards and forwards, over rough ground and smooth. The classical style of memoir, after the manner of Lagrange, or Poisson, or Gauss, complete in itself and deliberately composed like a work of art, is continually becoming rarer. It is therefore more and more essential that from time to time some one should come forward to sort out and arrange the accumulated material, rejecting what has proved unimportant, and welding the rest into a connected system. There is perhaps a tendency to assume that such work is of secondary importance, and can be safely left to subordinate hands. But in reality it makes severe demands on even the highest powers; and when these have been available the result has often done more for the

progress of science than the composition of a dozen monographs on isolated points. For proof one need only point to the treatises of Salmon himself, or recall (in another field) the debt which we owe to such books as the 'Treatise on Natural Philosophy' and the 'Theory of Sound,' whose authors are happily with us.

A modest but most valuable worker has passed away in the person of Professor Allman. His treatise on the history of Greek geometry, full of learning and sound mathematical perception, is written with great simplicity and an entire absence of pedantry or dogmatism. It ranks, I believe, with the best that has been done on the subject. It is to be regretted that, as an historian, he leaves so few successors among British mathematicians. We have amongst us, as a result of our system of university education, many men of trained mathematical faculty and of a scholarly turn of mind, with much of the necessary linguistic equipment, who feel, however, no special vocation for the details of recent mathematical research. Might not some of this ability be turned to a field, by no means exhausted, where the severity of mathematical truth is tempered by the human interest attaching to the lives, the vicissitudes and even the passions and the strife of its devotees, who through many errors and perplexities have contrived to keep alive and trim the sacred flame, and to hand it on burning ever clearer and brighter?

Of the various subjects which fall within the scope of this section there is no difficulty in naming that which at the present time excites the widest interest. The phenomena of radioactivity, ionization of gases, and so on, are not only startling and sensational in themselves, they have suggested most wonderful and far-reaching speculations, and, whatever be the future of these particular theories, they are bound in any case deeply to influence our views on fundamental points of chemistry and physics. No reference to this subject would, I think, be satisfactory without a word of homage to the unsurpassed patience and skill in the devising of new experimental methods to meet new and subtle conditions which it has evoked. It will be felt, as a matter of legitimate pride, by many present, that the University of Cambridge has been so conspicuously associated with this work. It would therefore have been natural and appropriate that this chair should have been occupied, this year above others, by one who could have given us a survey of the facts as they at present stand, and of their bearing, so far as can be discerned, on other and older branches of physics. Whether from the experimental or from the more theoretical and philosophical standpoint, there would have been no difficulty in finding exponents of unrivaled authority. But it has been otherwise ordered, and you and I must make the best of it. If the subject can not be further dealt with for the moment, we have the satisfaction of know-

ing that it will in due course engage the attention of the section, and that we may look forward to interesting and stimulating discussions, in which we trust the many distinguished foreign physicists who honor us by their presence will take an active part.

It is, I believe, not an unknown thing for your president to look up the records of previous meetings in search of inspiration, and possibly of an example. I have myself not had to look very far, for I found that when the British Association last met in Cambridge, in the year 1862, this section was presided over by Stokes, and moreover that the address which he gave was probably the shortest ever made on such an occasion, for it occupies only half a page of the report, and took, I should say, some three or four minutes to deliver. It would be to the advantage of the business of the meeting, and to my own great relief, if I had the courage to follow so attractive a precedent; but I fear that the tradition which has since established itself is too strong for me to break without presumption. I will turn, therefore, in the first instance, to a theme which, I think, naturally presents itself—viz., a consideration of the place occupied by Stokes in the development of mathematical physics. It is not proposed to attempt an examination or appreciation of his own individual achievements; this has lately been done by more than one hand, and in the most authoritative manner. But it is part of the greatness of the man that his work can be reviewed from more than one standpoint. What I specially wish to direct attention to on this occasion is the historical or evolutionary relation in which he stands to predecessors and followers in the above field.

The early years of Stokes's life were the closing years of a mighty generation of mathematicians and mathematical physicists. When he came to manhood, Lagrange, Laplace, Poisson, Fourier, Fresnel, Ampère had but recently passed away. Cauchy alone of this race of giants was still alive and productive. It is upon these men that we must look as the immediate intellectual ancestors of Stokes, for, although Gauss and F. Neumann were alive and flourishing, the interaction of German and English science was at that time not very great. It is noteworthy, however, that the development of the modern German school of mathematical physics, represented by Helmholtz and Kirchoff, in linear succession to Neumann, ran in many respects closely parallel to the work of Stokes and his followers.

When the foundations of analytical dynamics had been laid by Euler and d'Alembert, the first important application was naturally to the problems of gravitational astronomy; this formed, of course, the chief work of Laplace, Lagrange and others. Afterwards came the theoretical study of elasticity, conduction of heat, statical electricity, and magnetism. The investigations in elasticity were undertaken mainly in relation to physical optics, with the hope of finding a mate-

rial medium capable of conveying transverse vibrations, and of accounting also for the various phenomena of reflection, refraction and double refraction. It has often been pointed out, as characteristic of the French school referred to, that their physical speculations were largely influenced by ideas transferred from astronomy; as, for instance, in the conception of a solid body as made up of discrete particles acting on one another at a distance with forces in the lines joining them, which formed the basis of most of their work on elasticity and optics. The difficulty of carrying out these ideas in a logical manner was enormous, and the strict course of mathematical deduction had to be replaced by more or less precarious assumptions. The detailed study of the geometry of a continuous deformable medium which was instituted by Cauchy was a first step towards liberating the theory from arbitrary and unnecessary hypothesis; but it was reserved for Green, the immediate predecessor of Stokes among English mathematicians, to carry out this process completely and independently, with the help of Lagrange's general dynamical methods, which here found their first application to questions of physics outside the ordinary dynamics of rigid bodies and fluids. The modern school of English physicists, since the time of Green and Stokes, have consistently endeavored to make out, in any given class of phenomena, how much can be recognized as a manifestation of general dynamical principles, independent of the particular mechanism which may be at work. One of the most striking examples of this was the identification by Maxwell of the laws of electromagnetism with the dynamical equations of Lagrange. It would, however, be going too far to claim this tendency as the exclusive characteristic of English physicists; for example, the elastic investigations of Green and Stokes have their parallel in the independent though later work of Kirchhoff; and the beautiful theory of dynamical systems with latent motion which we owe to Lord Kelvin stands in a very similar relation to the work of Helmholtz and Hertz.

But perhaps the most important and characteristic feature in the mathematical work of the later school is its increasing relation to and association with experiment. In the days when the chief applications of mathematics were to the problems of gravitational astronomy, the mathematician might well take his materials at second hand; and in some respects the division of labor was, and still may be, of advantage. The same thing holds in a measure of the problems of ordinary dynamics, where some practical knowledge of the subject matter is within the reach of every one. But when we pass to the more recondite phenomena of physical optics, acoustics and electricity, it hardly needs the demonstrations which have involuntarily been given to show that the theoretical treatment must tend to degenerate into the pursuit of mere academic subtleties unless it is constantly vivified by direct con-

tact with reality. Stokes, at all events, with little guidance or encouragement from his immediate environment, made himself from the first practically acquainted with the subjects he treated. Generations of Cambridge students recall the enthusiasm which characterized his experimental demonstrations in optics. These appealed to us all; but some of us, I am afraid, under the influence of the academic ideas of the time, thought it a little unnecessary to show practically that the height of the lecture-room could be measured by the barometer, or to verify the calculated period of oscillation of water in a tank by actually timing the waves with the help of the image of a candle-flame reflected at the surface.

The practical character of the mathematical work of Stokes and his followers is shown especially in the constant effort to reduce the solution of a physical problem to a quantitative form. A conspicuous instance is furnished by the labor and skill which he devoted, from this point of view, to the theory of the Bessel's function, which presents itself so frequently in important questions of optics, electricity and acoustics, but is so refractory to ordinary methods of treatment. It is now generally accepted that an analytical solution of a physical question, however elegant it may be made to appear by means of a judicious notation, is not complete so long as the results are given merely in terms of functions defined by infinite series or definite integrals, and can not be exhibited in a numerical or graphical form. This view did not originate, of course, with Stokes; it is clearly indicated, for instance, in the works of Fourier and Poinsot, but no previous writer had, I think, acted upon it so consistently and thoroughly.

We have had so many striking examples of the fruitfulness of the combination of great mathematical and experimental powers that the question may well be raised, whether there is any longer a reason for maintaining in our minds a distinction between mathematical and experimental physics, or at all events whether these should be looked upon as separate provinces which may conveniently be assigned to different sets of laborers. It may be held that the highest physical research will demand in the future the possession of both kinds of faculty. We must be careful, however, how we erect barriers which would exclude a Lagrange on the one side or a Faraday on the other. There are many mansions in the palace of physical science, and work for various types of mind. A zealous, or over-zealous, mathematician might indeed make out something of a case if he were to contend that, after all, the greatest work of such men as Stokes, Kirchhoff and Maxwell was mathematical rather than experimental in its complexion. An argument which asks us to leave out of account such things as the investigation of fluorescence, the discovery of spectrum analysis and the measurement of the viscosity of gases, may seeem audacious; but a

survey of the collected works of these writers will show how much, of the very highest quality and import, would remain. However this may be, the essential point, which can not, I think, be contested, is this, that if these men had been condemned and restricted to a mere book knowledge of the subjects which they have treated with such marvelous analytical ability, the very soul of their work would have been taken away. I have ventured to dwell upon this point because, although I am myself disposed to plead for the continued recognition of mathematical physics as a fairly separate field, I feel strongly that the traditional kind of education given to our professed mathematical students does not tend to its most effectual cultivation. This education is apt to be one-sided, and too much divorced from the study of tangible things. Even the student whose tastes lie mainly in the direction of pure mathematics would profit, I think, by a wider scientific training. A long list of instances might be given to show that the most fruitful ideas in pure mathematics have been suggested by the study of physical problems. In the words of Fourier, who did so much to fulfil his own saying, "L'étude approfondie de la nature est la source la plus féconde des découvertes mathématiques. Non-seulement cette étude, en offrant aux recherches un but déterminé, a l'avantage d'exclure les questions vagues et les calculs sans issue; elle est encore un moyen assuré de former l'analyse elle-même, et d'en découvrir les éléments qu'il nous importe le plus de connaître, et que cette science doit toujours conserver: ces éléments fondamentaux sont ceux qui se reproduisent dans tous les effets naturels."

Another characteristic of the applied mathematics of the past century is that it was, on the whole, the age of linear equations. The analytical armory fashioned by Lagrange, Poisson, Fourier and others, though subject, of course, to continual improvement and development, has served the turn of a long line of successors. The predominance of linear equations, in most of the physical subjects referred to, rests on the fact that the changes are treated as infinitely small. The electric theory of light forms at present an exception; but even here the linear character of the fundamental electrical relations is itself remarkable, and possibly significant. The theory of small oscillations, in particular, runs as a thread through a great part of the literature of the period in question. It has suggested many important analytical results, and it still gives the best and simplest intuitive foundation for a whole class of theorems which are otherwise hard to comprehend in their various relations, such as Fourier's theorem, Laplace's expansion, Bessel's functions, and the like. Moreover, the interest of the subject, whether mathematical or physical, is not yet exhausted; many important problems in optics and acoustics, for example, still await solution. The general theory has in comparatively recent times re-

ceived an unexpected extension (to the case of 'latent motions') at the hands of Lord Kelvin; and Lord Rayleigh, by his continual additions to it, shows that, in his view, it is still incomplete.

When the restriction to infinitely small motions is abandoned, the problems become of course much more arduous. The whole theory, for instance, of the normal modes of vibration which is so important in acoustics, and even in music, disappears. The researches hitherto made in this direction have, moreover, encountered difficulties of a less patent character. It is conceivable that the modern analytical methods which have been developed in astronomy may have an application to these questions. It would appear that there is an opening here for the mathematician; at all events, the numerical or graphical solution of any one of the various problems that could be suggested would be of the highest interest. One problem of the kind is already classical—the theory of steep water-waves discussed by Stokes; but even here the point of view has perhaps been rather artificially restricted. The question proposed by him, the determination of the possible forms of waves of permanent type, like the problem of periodic orbits in astronomy, is very interesting mathematically, and forms a natural starting-point for investigation; but it does not exhaust what is most important for us to know in the matter. Observation may suggest the existence of such waves as a fact; but no reason has been given, so far as I know, why free water-waves should tend to assume a form consistent with permanence, or be influenced in their progress by considerations of geometrical simplicity.

I have tried to indicate the kind of continuity of subject-matter, method and spirit which runs through the work of the whole school of mathematical physicists of which Stokes may be taken as the representative. It is no less interesting, I think, to examine the points of contrast with more recent tendencies. These relate not so much to subject matter and method as to the general mental attitude towards the problems of nature. Mathematical and physical science have become markedly introspective. The investigators of the classical school, as it may perhaps be styled, were animated by a simple and vigorous faith; they sought as a matter of course for a mechanical explanation of phenomena, and had no misgivings as to the trustiness of the analytical weapons which they wielded. But now the physicist and the mathematician alike are in trouble about their souls. We have discussions on the principles of mechanics, on the foundations of geometry, on the logic of the most rudimentary arithmetical processes, as well as the more artificial operations of the calculus. These discussions are legitimate and inevitable, and have led to some results which are now widely accepted. Although they were carried on to a great extent independently, the questions involved will, I think, be found to

be ultimately very closely connected. Their common nexus is, perhaps, to be traced in the physiological ideas of which Helmholtz was the most conspicuous exponent. To many minds such discussions are repellant, in that they seem to venture on the uncertain ground of philosophy. But, as a matter of fact, the current views on these subjects have been arrived at by men who have gone to work in their own way, often in entire ignorance of what philosophers have thought on such subjects. It may be maintained chiefly, indeed, that the mathematician or the physicist, as such, has no special concern with philosophy, any more than the engineer or the geographer. Nor, although this is a matter for their own judgment, would it appear that philosophers have very much to gain by a special study of the methods of mathematical or physical reasoning, since the problems with which they are concerned are presented to them in a much less artificial form in the circumstances of ordinary life. As regards the present topic I would put the matter in this way, that between mathematics and physics on the one hand and philosophy on the other there lies an undefined borderland, and that the mathematician has been engaged in setting things in order, as he is entitled to do, on his own side of the boundary.

Adopting this point of view, it would be of interest to trace in detail the relationships of the three currents of speculation which have been referred to. At one time, indeed, I was tempted to take this as the subject of my address; but, although I still think the enterprise a possible one, I have been forced to recognize that it demands a better equipment than I can pretend to. I can only venture to put before you some of my tangled thoughts on the matter, trusting that some future occupant of this chair may be induced to take up this question and treat it in a more illuminating manner.

If we look back for a moment to the views currently entertained not so very long ago by mathematicians and physicists, we shall find, I think, that the prevalent conception of the world was that it was constructed on some sort of absolute geometrical plan, and that the changes in it proceeded according to precise laws; that, although the principles of mechanics might be imperfectly stated in our text-books, at all events such principles existed, and were ascertainable, and, when properly formulated, would possess the definiteness and precision which were held to characterize, say, the postulates of Euclid. Some writers have maintained, indeed, that the principles in question were finally laid down by Newton, and have occasionally used language which suggests that any fuller understanding of them was a mere matter of interpretation of the text. But, as Hertz has remarked, most of the great writers on dynamics betray, involuntarily, a certain *malaise* when explaining the principles, and hurry over this part of their task as quickly as is consistent with dignity. They are not really at their ease until,

having established their equations somehow, they can proceed to build securely on these. This has led some people to the view that the laws of nature are merely a system of differential equations; it may be remarked in passing that this is very much the position in which we actually stand in some of the more recent theories of electricity. As regards dynamics, when once the critical movement had set in, it was easy to show that one presentation after another was logically defective and confused; and no satisfactory standpoint was reached until it was recognized that in the classical dynamics we do not deal immediately with real bodies at all, but with certain conventional and highly idealized representations of them, which we combine according to arbitrary rules, in the hope that if these rules be judiciously framed the varying combinations will image to us what is of most interest in some of the simpler and more important phenomena. The changed point of view is often associated with the publication of Kirchhoff's lectures on mechanics in 1876, where it is laid down in the opening sentence that the problem of mechanics is to describe the motions which occur in nature completely and in the simplest manner. This statement must not be taken too literally; at all events, a fuller, and I think a clearer, account of the province and method of abstract dynamics is given in a review of the second edition of Thomson and Tait, which was one of the last things penned by Maxwell in 1879.* A 'complete' description of even the simplest natural phenomenon is an obvious impossibility; and, were it possible, it would be uninteresting as well as useless, for it would take an incalculable time to peruse. Some process of selection and idealization is inevitable if we are to gain any intelligent comprehension of events. Thus, in astronomy we replace a planet by a so-called material particle—*i. e.*, a mathematical point associated with a suitable numerical coefficient. All the properties of the body are here ignored except those of position and mass, in which alone we are at the moment interested. The whole course of physical sciences and the language in which its results are expressed have been largely determined by the fact that the ideal images of geometry were already at hand at its service. The ideal representations have the advantage that, unlike the real objects, definite and accurate statements can be made about them. Thus two lines in a geometrical figure can be pronounced to be equal or unequal, and the statement is in either case absolute. It is no doubt hard to divest oneself entirely of the notion conveyed in the Greek phrase *ἀεὶ ὁ θεὸς γεωμετρεῖ*, that definite geometrical magnitudes and relations are at the back of phenomena. It is recognized indeed that all our measurements are necessarily to some degree uncertain, but this is usually attributed to our own limitations and those of our instruments rather than to the ultimate vagueness of the entity

* *Nature*, Vol. XX., p. 213; *Scientific Papers*, Vol. II., p. 776.

which it is sought to measure. Every one will grant, however, that the distance between two clouds, for instance, is not a definable magnitude; and the distance of the earth from the sun, and even the length of a wave of light, are in precisely the same case. The notion in question is a convenient fiction, and is a striking testimony to the ascendency which Greek mathematics have gained over our minds, but I do not think that more can be said for it. It is, at any rate, not verified by the experience of those who actually undertake physical measurements. The more refined the means employed, the more vague and elusive does the supposed magnitude become; the judgment flickers and wavers, until at last in a sort of despair *some* result is put down, not in the belief that it is exact, but with the feeling that it is the best we can make of the matter. A practical measurement is in fact a classification; we assign a magnitude to a certain category, which may be narrowly limited, but which has in any case a certain breadth.

By a frank process of idealization a logical system of abstract dynamics can doubtless be built up, on the lines sketched by Maxwell in the passage referred to. Such difficulties as remain are handed over to geometry. But we can not stop in this position; we are constrained to examine the nature and the origin of the conceptions of geometry itself. By many of us, I imagine, the first suggestion that these conceptions are to be traced to an empirical source was received with something of indignation and scorn; it was an outrage on the science which we had been led to look upon as divine. Most of us have, however, been forced at length to acquiesce in the view that geometry, like mechanics, is an applied science; that it gives us merely an ingenious and convenient symbolic representation of the relations of actual bodies; and that, whatever may be the *a priori* forms of intuition, the science as we have it could never have been developed except for the accident (if I may so term it) that we live in a world in which rigid or approximately rigid bodies are conspicuous objects. On this view the most refined geometrical demonstration can be resolved into a series of imagined experiments performed with such bodies, or rather with their conventional representations.

It is to be lamented that one of the most interesting chapters in the history of science is a blank; I mean that which would have unfolded the rise and growth of our system of ideal geometry. The finished edifice is before us, but the record of the efforts by which the various stones were fitted into their places is hopelessly lost. The few fragments of professed history which we possess were edited long after the achievement.

It is commonly reckoned that the first rude beginnings of geometry date from the Egyptians. I am inclined to think that in one sense the matter is to be placed much further back, and that the dawn of geo-

metric ideas is to be traced among the prehistoric races who carved rough but thoroughly artistic outlines of animals on their weapons. I do not know whether the matter has attracted serious speculation, but I have myself been led to wonder how men first arrived at the notion of an outline drawing. The primitive sketches referred to immediately convey to the experienced mind the idea of a reindeer or the like; but in reality the representation is purely conventional, and is expressed in a language which has to be learned. For nothing could be more unlike the actual reindeer than the few scratches drawn on the surface of a bone; and it is of course familiar to ourselves that it is only after a time, and by an insensible process of education, that very young children come to understand the meaning of an outline. Whoever he was, the man who first projected the world into two dimensions, and proceeded to fence off that part of it which was reindeer from that which was not, was certainly under the influence of a geometrical idea, and had his feet in the path which was to culminate in the refined idealizations of the Greeks. As to the manner in which these latter were developed, the only indication of tradition is that some propositions were arrived at first in a more empirical or intuitional, and afterwards in a more intellectual way. So long as points had size, lines had breadth and surfaces thickness, there could be no question of exact relations between the various elements of a figure, any more than is the case with the realities which they represent. But the Greek mind loved definiteness, and discovered that if we agree to speak of lines *as if* they had no breadth, and so on, exact statements became possible. If any one scientific invention can claim preeminence over all others, I should be inclined myself to erect a monument to the inventor of the mathematical point, as the supreme type of that process of abstraction which has been a necessary condition of scientific work from the very beginning.

It is possible, however, to uphold the importance of the part which abstract geometry has played, and must still play, in the evolution of scientific conceptions, without committing ourselves to a defense, on all points, of the traditional presentment. The consistency and completeness of the usual system of definitions, axioms and postulates have often been questioned; and quite recently a more thoroughgoing analysis of the logical elements of the subject than has ever before been attempted has been made by Hilbert. The matter is a subtle one, and a general agreement on such points is as yet hardly possible. The basis for such an agreement may perhaps ultimately be found in a more explicit recognition of the empirical source of the fundamental conceptions. This would tend, at all events, to mitigate the rigor of the demands which are sometimes made for logical perfection.

Even more important in some respects are the questions which have

arisen in connection with the applications of geometry to purposes of graphical representation. It is not necessary to dwell on the great assistance which this method has rendered in such subjects as physics and engineering. The pure mathematician, for his part, will freely testify to the influence which it has exercised in the development of most branches of analysis; for example, we owe to it all the leading ideas of the calculus. Modern analysts have discovered, however, that geometry may be a snare as well as a guide. In the mere act of drawing a curve to represent an analytical function we make unconsciously a host of assumptions which are difficult not merely to prove, but even to formulate precisely. It is now sought to establish the whole fabric of mathematical analysis on a strictly arithmetical basis. To those who were trained in an earlier school, the results so far are in appearance somewhat forbidding. If the shade of one of the great analysts of a century ago could revisit the glimpses of the moon, his feelings would, I think, be akin to those of the traveler to some medieval town, who finds the buildings he came to see obscured by scaffolding, and is told that the ancient monuments are all in process of repair. It is to be hoped that a good deal of this obstruction is only temporary, that most of the scaffolding will eventually be cleared away, and that the edifices when they reappear will not be entirely transformed, but will still retain something of their historic outlines. It would be contrary to the spirit of this address to undervalue in any way the critical examination and revision of principles; we must acknowledge that it tends ultimately to simplification, to the clearing up of issues, and the reconciliation of apparent contradictions. But it would be a misfortune if this process were to absorb too large a share of the attention of mathematicians, or were allowed to set too high a standard of logical completeness. In this particular matter of the 'arithmetization of mathematics' there is, I think, a danger in these respects. As regards the latter point, a traveler who refuses to pass over a bridge until he has personally tested the soundness of every part of it is not likely to go very far; something must be risked, even in mathematics. It is notorious that even in this realm of 'exact' thought discovery has often been in advance of strict logic, as in the theory of imaginaries, for example, and in the whole province of analysis of which Fourier's theorem is the type. And it might even be claimed that the services which geometry has rendered to other sciences have been almost as great in virtue of the questions which it implicitly begs as of those which it resolves.

I would venture, with some trepidation, to go one step further. Mathematicians love to build on as definite a foundation as possible, and from this point of view the notion of the integral number, on which (we are told) the mathematics of the future are to be based, is

very attractive. But, as an instrument for the study of nature, is it really more fundamental than the geometrical notions which it is to supersede? The accounts of primitive peoples would seem to show that, in the generality which is a necessary condition for this purpose, it is in no less degree artificial and acquired. Moreover, does not the act of enumeration, as applied to actual things, involve the very same process of selection and idealization which we have already met with in other cases? As an illustration, suppose we were to try to count the number of drops of water in a cloud. I am not thinking of the mere practical difficulties of enumeration, or even of the more pertinent fact that it is hard to say where the cloud begins or ends. Waiving these points, it is obvious that there must be transitional stages between a more or less dense group of molecules and a drop, and in the case of some of these aggregates it would only be by an arbitrary exercise of judgment that they would be assigned to one category rather than to the other. In whatever form we meet with it, the very notion of counting involves the highly artificial conception of a number of objects which for some purposes are treated as absolutely alike, whilst yet they can be distinguished.

The net result of the preceding survey is that the systems of geometry, of mechanics and even of arithmetic, on which we base our study of nature, are all contrivances of the same general kind: they consist of series of abstractions and conventions devised to represent, or rather to symbolize, what is most interesting and most accessible to us in the world of phenomena. And the progress of science consists in a great measure in the improvement, the development and the simplification of these artificial conceptions, so that their scope may be wider and the representation more complete. The best in this kind are but shadows, but we may continually do something to amend them.

As compared with the older view, the function of physical science is seen to be much more modest than was at one time supposed. We no longer hope by levers and screws to pluck out the heart of the mystery of the universe. But there are compensations. The conception of the physical world as a mechanism, constructed on a rigid mathematical plan, whose most intimate details might possibly some day be guessed, was, I think, somewhat depressing. We have been led to recognize that the formal and mathematical element is of our own introduction; that it is merely the apparatus by which we map out our knowledge, and has no more objective reality than the circles of latitude and longitude on the sun. A distinguished writer not very long ago speculated on the possibility of the scientific mine being worked out within no distant period. Recent discoveries seem to have put back this possibility indefinitely; and the tendency of modern speculation as to the nature of scientific knowledge should be to banish it altogether.

The world remains a more wonderful place than ever; we may be sure that it abounds in riches not yet dreamed of; and although we can not hope ever to explore its innermost recesses, we may be confident that it will supply tasks in abundance for the scientific mind for ages to come.

One significant result of the modern tendency is that we no longer with the same obstinacy demand a mechanical explanation of the phenomena of light and electricity, especially since it has been made clear that if one mechanical explanation is possible, there will be an infinity of others. Some minds, indeed, reveling in their new-found freedom, have attempted to disestablish ordinary or 'vulgar' matter altogether. I may refer to a certain treatise which, by some accident, does not bear its proper title of 'Æther and *no* Matter,' and to the elaborate investigations of Professor Osborne Reynolds, which present the same peculiarity, although the basis is different. Speculations of this nature have, however, been so recently and (if I may say it) so brilliantly dealt with by Professor Poynting before this section that there is little excuse for dwelling further on them now. I will only advert to the question whether, as some suggest, physical science should definitely abandon the attempt to construct mechanical theories in the older sense. The question would appear to be very similar to this, whether we should abandon the use of graphical methods in analysis. In either case we run the risk of introducing extraneous elements, possibly of a misleading character; but the gain in vividness of perception and in suggestiveness is so great that we are not likely altogether to forego it, by excess of prudence, in one case more than in the other.

We have traveled some distance from Stokes and the mathematical physics of half a century ago. May I add a few observations which might perhaps have claimed his sympathy? They are in substance anything but new, although I do not find them easy to express. We have most of us frankly adopted the empirical attitude in physical science; it has justified itself abundantly in the past, and has more and more forced itself upon us. We have given up the notion of causation, except as a convenient phrase; what were once called laws of nature are now simply rules by which we can tell more or less accurately what will be the consequences of a given state of things. We can not help asking, How is it that such rules are possible? A rule is invented in the first instance to sum up in a compact form a number of past experiences; but we apply it with little hesitation, and generally with success, to the prediction of new and sometimes strange ones. Thus the law of gravitation indicates the existence of Neptune; and Fresnel's wave-surface gives us the quite unsuspected phenomenon of double refraction. Why does nature make a point of honoring our

cheques in this manner; or, to put the matter in a more dignified form, how comes it that, in the words of Schiller,*

> 'Mit dem Genius steht die Natur im ewigen Bunde,
> Was der eine verspricht, leistet die andre gewiss'?

The question is as old as science, and the modern tendencies with which we have been occupied have only added point to it. It is plain that physical science has no answer; its policy indeed has been to retreat from a territory which it could not securely occupy. We are told in some quarters that it is vain to look for an answer anywhere. But the mind of man is not wholly given over to physical science, and will not be content forever to leave the question alone. It will persist in its obstinate questionings, and, however hopeless the attempt to unravel the mystery may be deemed, physical science, powerless to assist, has no right to condemn it.

I would like, in conclusion, to read to you a characteristic passage from that address of Stokes in 1862 which has formed the starting-point of this discourse:

"In this section, more perhaps than in any other, we have frequently to deal with subjects of a very abstract character, which in many cases can be mastered only by patient study, at leisure, of what has been written. The question may not unnaturally be asked, If investigations of this kind can best be followed by quiet study in one's own room, what is the use of bringing them forward in a sectional meeting at all? I believe that good may be done by public mention, in a meeting like the present, of even somewhat abstract investigations; but whether good is thus done, or the audience merely wearied to no purpose, depends upon the judiciousness of the person by whom the investigation is brought forward."

It might be urged that these remarks are as pertinent now as they were forty years ago, but I will leave them on their own weighty authority. I will not myself attempt to emphasize them, lest some of my hearers should be tempted to retort that the warning might well be borne in mind, not only in the ordinary proceedings of the section, but in the composition of a presidential address!

* Applied by Herschel to the discovery of Neptune.

HEREDITY AND EVOLUTION.

By WILLIAM BATESON, M.A., F.R.S.,

PRESIDENT OF THE ZOOLOGICAL SECTION OF THE BRITISH ASSOCIATION.

IN choosing a subject for this address I have availed myself of the kindly usage which permits a sectional president to divert the attention of his hearers into those lines of inquiry which he himself is accustomed to pursue. Nevertheless, in taking the facts of breeding for my theme, I am sensible that this privilege is subjected to a certain strain.

Heredity—and variation too—are matters of which no naturalist likes to admit himself entirely careless. Every one knows that, somewhere hidden among the phenomena denoted by these terms, there must be principles which, in ways untraced, are ordering the destinies of living things. Experiments in heredity have thus, as I am told, a universal fascination. All are willing to offer an outward deference to these studies. The limits of that homage, however, are soon reached, and, though all profess interest, few are impelled to make even the moderate mental effort needed to apprehend what has been already done. It is understood that heredity is an important mystery, and variation another mystery. The naturalist, the breeder, the horticulturist, the sociologist, man of science and man of practise alike, have daily occasion to make and to act on assumptions as to heredity and variation, but many seem well content that such phenomena should remain forever mysterious.

The position of these studies is unique. At once fashionable and neglected, nominally the central common ground of botany and zoology, of morphology and physiology, belonging specially to neither, this area is thinly tenanted. Now, since few have leisure for topics with which they can not suppose themselves concerned, I am aware that, when I ask you in your familiar habitations to listen to tales of a no man's land, I must forego many of those supports by which a speaker may maintain his hold on the intellectual sympathy of an audience.

Those whose pursuits have led them far from their companions can not be exempt from that differentiation which is the fate of isolated groups. The stock of common knowledge and common ideas grows smaller till the difficulty of intercommunication becomes extreme. Not only has our point of view changed, but our materials are unfamiliar, our methods of inquiry new, and even the results attained accord little with the common expectations of the day. In the progress of sciences

we are used to be led from the known to the unknown, from the half-perceived to the proven, the expectation of one year becoming the certainty of the next. It will aid appreciation of the change coming over evolutionary science if it be realized that the new knowledge of heredity and variation rather replaces than extends current ideas on those subjects.

Convention requires that a president should declare all well in his science; but I can not think it a symptom indicative of much health in our body that the task of assimilating the new knowledge has proved so difficult. An eminent foreign professor lately told me that he believed there were not half a dozen in his country conversant with what may be called Mendelism, though he added hopefully, 'I find these things interest my students more than my colleagues.' A professed biologist can not afford to ignore a new life history, the Okapi, or the other last new version of the old story; but phenomena which put new interpretations on the whole, facts witnessed continually by all who are working in these fields, he may conveniently disregard as matters of opinion. Had a discovery comparable in magnitude with that of Mendel been announced in physics or in chemistry, it would at once have been repeated and extended in every great scientific school throughout the world. We could come to a British Association audience to discuss the details of our subject—the polymorphism of extracted types, the physiological meaning of segregation, its applicability to the case of sex, the nature of non-segregable characters, and like problems with which we are now dealing—sure of finding sound and helpful criticism, nor would it be necessary on each occasion to begin with a popular presentation of the rudiments. This state of things in a progressive science has arisen, as I think, from a loss of touch with the main line of inquiry. The successes of descriptive zoology are so palpable and so attractive, that, not unnaturally, these which are the means of progress have been mistaken for the end. But now that the survey of terrestrial types by existing methods is happily approaching completion, we may hope that our science will return to its proper task, the detection of the fundamental nature of living things. I say *return*, because, in spite of that perfecting of the instruments of research characteristic of our time, and an extension of the area of scrutiny, the last generation was nearer the main quest. No one can study the history of biology without perceiving that in some essential respects the spirit of the naturalists of fifty years ago was truer in aim, and that their methods of inquiry were more direct and more fertile—so far, at least, as the problem of evolution is concerned—than those which have replaced them.

If we study the researches begun by Kölreuter and continued with great vigor till the middle of the sixties, we can not fail to see that, had the experiments he and his successors undertook been continued

on the same lines, we should by now have advanced far into the unknown. More than this: if a knowledge of what those men actually accomplished had not passed away from the memory of our generation, we should now be able to appeal to an informed public mind, having some practical acquaintance with the phenomena, and possessing sufficient experience of these matters to recognize absurdity in statement and deduction, ready to provide that healthy atmosphere of instructed criticism most friendly to the growth of truth.

Elsewhere I have noted the paradox that the appearance of the work of Darwin, which crowns the great period in the study of the phenomena of species, was the signal for a general halt. The 'Origin of Species,' the treatise which for the first time brought the problem of species fairly within the range of human intelligence, so influenced the course of scientific thought that the study of this particular phenomenon—specific difference—almost entirely ceased. That this was largely due to the simultaneous opening up of lines of research in many other directions may be granted; but in greater measure, I believe, it is to be ascribed to the substitution of a conception of species which, with all the elements of truth it contains, is yet barren and unnatural. It is not wonderful that those who held that specific difference must be a phenomenon of slowest accumulation, proceeding by steps needing generations for their perception, should turn their attention to subjects deemed more amenable to human enterprise.

The indiscriminate confounding of all divergences from type into one heterogeneous heap under the name 'Variation' effectually concealed those features of order which the phenomena severally present, creating an enduring obstacle to the progress of evolutionary science. Specific normality and distinctness being regarded as an accidental product of exigency, it was thought safe to treat departures from such normality as comparable differences: all were 'variations' alike. Let us illustrate the consequences. Princess of Wales is a large modern violet, single, with stalks a foot long or more. Marie Louise is another, with large double flowers, pale color, short stalks, peculiar scent, leaf, etc. We call these 'varieties,' and we speak of the various fixed differences between these two, and between them and wild *odorata*, as due to variation; and, again, the transient differences between the same *odorata* in poor, dry soil, or in a rich hedge-bank, we call variation, using but the one term for differences, quantitative or qualitative, permanent or transitory, in size, number of parts, chemistry, and the rest. We might as well use one term to denote the differences between a bar of silver, a stick of lunar caustic, a shilling or a teaspoon. No wonder that the ignorant tell us they can find no order in variation.

This prodigious confusion, which has spread obscurity over every part of these inquiries, is traceable to the original misconception of the nature of specific difference, as a thing imposed and not inherent.

From this, at least, the earlier experimenters were free; and the undertakings of Gärtner and his contemporaries were informed by the true conception that the properties and behavior of species were themselves specific. Free from the later fancy that but for selection the forms of animals and plants would be continuous and indeterminate, they recognized the definiteness of species and variety, and boldly set themselves to work out case by case the manifestations and consequences of that definiteness.

Over this work of minute and largely experimental analysis, rapidly growing, the new doctrine that organisms are mere conglomerates of adaptative devices descended like a numbing spell. By an easy confusion of thought, faith in the physiological definiteness of species and variety passed under the common ban which had at last exorcised the demon immutability. Henceforth no naturalist must hold communion with either, on pain of condemnation as an apostate, a danger to the dynasty of selection. From this oppression we in England, at least, are scarcely beginning to emerge. Bentham's 'Flora,' teaching very positively that the primrose, the cowslip, and the oxlip are impermanent varieties of one species, is in the hand of every beginner, while the British Museum reading-room finds it unnecessary to procure Gärtner's '*Bastarderzeugung.*'

And so this mass of specific learning has passed out of account. The evidence of the collector, the horticulturist, the breeder, the fancier, has been treated with neglect, and sometimes, I fear, with contempt. That wide field whence Darwin drew his wonderful store of facts has been some forty years untouched. Speak to professional zoologists of any breeder's matter, and how many will not intimate to you politely that fanciers are unscientific persons, and their concerns beneath notice? For the concrete in evolution we are offered the abstract. Our philosophers debate with great fluency whether between imaginary races sterility could grow up by an imaginary selection; whether selection working upon hypothetical materials could produce sexual differentiation; how under a system of natural selection bodily symmetry may have been impressed on formless protoplasm—that monstrous figment of the mind, fit starting-point for such discussions. But by a physiological irony enthusiasm for these topics is sometimes fully correlated with indifference even to the classical illustrations; and for many whose minds are attracted by the abstract problem of interracial sterility there are few who can name for certain ten cases in which it has been already observed.

And yet in the natural world, in the collecting-box, the seed-bed, the poultry-yard, the places where variation, heredity, selection, may be seen in operation and their properties tested, answers to these questions meet us at every turn—fragmentary answers, it is true, but each direct to the point. For if any one will stoop to examine nature in

those humble places, will do a few days' weeding, prick out some rows of cabbages, feed up a few score of any variable larva, he will not wait long before he learns the truth about variation. If he go further and breed two or three generations of almost any controllable form, he will obtain immediately facts as to the course of heredity which obviate the need for much laborious imagining. If strictly trained, with faith in the omnipotence of selection, he will not proceed far before he encounters disquieting facts. Upon whatever character the attention be fixed, whether size, number, form of the whole or of the parts, proportion, distribution of differentiation, sexual characters, fertility, precocity or lateness, color, susceptibility to cold or to disease—in short, all the kinds of characters which we think of as best exemplifying specific difference, we are certain to find illustrations of the occurrence of departures from normality, presenting exactly the same definiteness elsewhere characteristic of normality itself. Again and again the circumstances of their occurrence render it impossible to suppose that these striking differences are the product of continued selection, or, indeed, that they represent the results of a gradual transformation of any kind. Whenever by any collocation of favoring circumstance such definite novelties possess a superior viability, supplanting their 'normal' relatives, it is obvious that new types will be created.

The earliest statement of this simple inference is, I believe, that of Marchant,* who in 1719, commenting on certain plants of *Mercurialis* with laciniated and hair-like leaves, which for a time established themselves in his garden, suggested that species may arise in like manner. Though the same conclusion has appeared inevitable to many, including authorities of very diverse experience, such as Huxley, Virchow, F. Galton, it has been strenuously resisted by the bulk of scientific opinion, especially in England. Lately, however, the belief in mutation, as De Vries has taught us to call it, has made notable progress,† owing to the publication of his splendid collection of observations and experiments, which must surely carry conviction of the reality and abundance of mutation to the minds of all whose judgments can be affected by evidence.

That the dread test of natural selection must be passed by every aspirant to existence, however brief, is a truism which needs no special

* Marchant, *Mém. Ac. roy. des sci.* for 1719; 1721, p. 59, Pls. 6–7. I owe this reference to Coutagne, *L'hérédité chez les vers à soie* (*Bull. sci. Fr. Belg.*, 1902).

† This progress threatens to be rapid indeed. Since these lines were written Professor Hubrecht, in an admirable exposition (POP. SCI. MONTHLY, July, 1904) of De Vries's 'Mutations-theorie,' has even blamed me for having ten years ago attached *any* importance to continuous variation. Nevertheless, when the unit of segregation is small, something mistakably like continuous evolution must surely exist. (Cp. Johannsen, 'Ueb. Erblichkeit in Populationen und in reinen Linien,' 1903.)

proof. Those who find satisfaction in demonstrations of the obvious may amply indulge themselves by starting various sorts of some annual, say French poppy, in a garden, letting them run to seed, and noticing in a few years how many of the finer sorts are represented; or by sowing an equal number of seeds taken from several varieties of carnation, lettuce or auricula, and seeing in what proportions the fine kinds survive in competition with the common.

Selection is a true phenomenon; but its function is to *select,* not to create. Many a white-edged poppy may have germinated and perished before Mr. Wilks saved the individual which in a few generations gave rise to the shirleys. Many a black *Amphidasys betularia* may have emerged before, some sixty years ago, in the urban conditions of Manchester the black var. *doubledayaria* found its chance, soon practically superseding the type in its place of origin, extending itself over England, and reappearing even in Belgium and Germany.

Darwin gave us sound teaching when he compared man's selective operations with those of nature. Yet how many who are ready to expound nature's methods have been at the pains to see how man really proceeds? To the domesticated form our fashions are what environmental exigency is to the wild. For years the conventional Chinese primrose threw sporadic plants of the loose-growing *stellata* variety, promptly extirpated because repugnant to mid-Victorian primness. But when taste, as we say, revived, the graceful star primula was saved by Messrs. Sutton, and a stock raised which is now of the highest fashion. I dare assert that few botanists meeting *P. stellata* in nature would hesitate to declare it a good species. This and the shirleys precisely illustrate the procedure of the raiser of novelties. His operations start from a definite beginning. As in the case of *P. stellata,* he may notice a mutational form thrown off perfect from the start, or, as in the shirleys, what catches his attention may be the first indication of that flaw which if allowed to extend will split the type into a host of new varieties each with its own peculiarities and physiological constitution.

Let any one who doubts this try what he can do by selection without such a definite beginning. Let him try from a pure strain of black and white rats to raise a white one by breeding from the whitest, or a black one by choosing the blackest. Let him try to raise a dwarf ('Cupid') sweet pea from a tall race by choosing the shortest, or a crested fowl by choosing the birds with most feathers on their heads. To formulate such suggestions is to expose their foolishness.

The creature is beheld to be very good after, not before its creation. Our domesticated races are sometimes represented as so many incarnations of the breeder's prophetic fancy. But except in recombinations of preexisting characters—now a comprehensible process—and in such intensifications and such finishing touches as involve variations

which analogy makes probable, the part played by prophecy is small. Variation leads; the breeder follows. The breeder's method is to notice a desirable novelty, and to work up a stock of it, picking up other novelties in his course—for these genetic disturbances often spread—and we may rest assured the method of nature is not very different.

The popular belief that evolution, whether natural or artificial, is effected by mass-selection of impalpable differences arises from many errors which are all phases of one—imperfect analysis—though the source of the error differs with the circumstances of its exponent. When the scientific advocate professes that he has statistical proofs of the continuity of variation, he is usually availing himself of that comprehensive use of the term variation to which I have referred. Statistical indications of such continuity are commonly derived from the study, not of nascent varieties, but of the fluctuations to which all normal populations are subject. Truly varying material needs care in its collection, and if found is often sporadic or in some other way unsuitable for statistical treatment. Sometimes it happens that the two phenomena are studied together in inextricable entanglement, and the resulting impression is a blur.

But when a practical man, describing his own experience, declares that the creation of his new breed has been a very long affair, the scientist, feeling that he has found a favorable witness, puts forward this testimony as conclusive. But on cross-examination it appears that the immense period deposed to seldom goes back beyond the time of the witness's grandfather, covering, say, seventy years; more often ten, or eight, or even five years will be found to have accomplished most of the business. Next, in this period—which, if we take it at seventy years, is a mere point of time compared with the epochs of which the selectionist discourses—a momentous transformation has often been effected, not in one character but many. Good characters have been added, it may be, of form, fertility, precocity, color and other physiological attributes, undesirable qualities have been eliminated, and all sorts of defects 'rogued' out. On analysis these operations can be proved to depend on a dozen discontinuities. Be it, moreover, remembered that within this period, besides *producing* his mutational character and combining it with other characters (or it may be groups of characters), the breeder has been working up a *stock*, reproducing in quantity that quality which first caught his attention, thus converting, if you will, a phenomenon of individuals into a phenomenon of a mass, to the future mystification of the careless.

Operating among such phenomena the gross statistical method is a misleading instrument; and, applied to these intricate discriminations, the imposing correlation table into which the biometrical Procrustes fits his arrays of unanalyzed data is still no substitute for the common sieve of a trained judgment. For nothing but minute analysis of the

facts by an observer thoroughly conversant with the particular plant or animal, its habits and properties, checked by the test of crucial experiment, can disentangle the truth.

To prove the reality of selection as a factor in evolution is, as I have said, a work of supererogation. With more profit may experiments be employed in defining the *limits* of what selection can accomplish. For whenever we can advance no further by selection, we strike that hard outline fixed by the natural properties of organisms. We come upon these limits in various unexpected places, and to the naturalist ignorant of breeding nothing can be more surprising or instructive.

Whatever be the mode of origin of new types, no theoretical evolutionist doubts that selection will enable him to fix his character when obtained. Let him put his faith into practise. Let him set about breeding canaries to win in the class for Clear Yellow Norwich at the Crystal Palace Show. Being a selectionist, his plan will be to pick up winning yellow cocks and hens at shows and breed them together. The results will be disappointing. Not getting what he wants, he may buy still better clear yellows and work them in, and so on till his funds are exhausted, but he will pretty certainly breed no winner, be he never so skilful. For no selection of winning yellows will make them into a breed. They must be formed afresh by various combinations of colors appropriately crossed and worked up. Though breeders differ as to the system of combinations to be followed, all would agree that selection of birds representing the winning type was a sure way to fail. The same is true for nearly all canary colors except in lizards, and, I believe, for some pigeon and poultry colors also.

Let this scientific fancier now go to the Palace Poultry Show and buy the winning brown leghorn cock and hen, breed from them, and send up the result of such a mating year after year. His chance of a winner is not quite, but almost *nil*. For in its wisdom the fancy has chosen one type for the cock and another for the hen. They belong to distinct strains. The hen corresponding to the winning cock is too bright, and the cock corresponding to the winning hen is too dull for the judge's taste. The same is the case in nearly every breed where the sex-colors differ markedly. Rarely winners of both sexes have come in one strain—a phenomenon I can not now discuss—but the contrary is the rule. Does any one suppose that this system of 'double mating' would be followed, with all the cost and trouble it involves, if selection could compress the two strains into one? Yet current theory makes demands on selection to which this is nothing.

The tyro has confidence in the power of selection to fix type, but he never stops to consider what fixation precisely means. Yet a simple experiment will tell him. He may go to a great show and claim the

best pair of Andalusian fowls for any number of guineas. When he breeds from them he finds, to his disgust, that only about half their chickens, or slightly more, come blue at all, the rest being blacks or splashed whites. Indignantly, perhaps, he will complain to the vendor that he has been supplied with no selected breed, but worthless mongrels. In reply he may learn that beyond a doubt his birds come from blues only in the direct line for an indefinite number of generations, and that to throw blacks and splashed whites is the inalienable property of blue Andalusians. But now let him breed from his 'wasters,' and he will find that the extracted blacks are *pure* and give blacks only, that the splashed whites similarly give only whites or splashed whites —but if the two sorts of 'wasters' are crossed together *blues only* will result. *Selection* will never make the blues breed true; nor can this ever come to pass unless a blue be found whose germ-cells are bearers of the blue character—which may or may not be possible. If the selectionist reflect on this experience he will be led straight to the center of our problem. There will fall, as it were, scales from his eyes, and in a flash he will see the true meaning of fixation of type, variability and mutation, vaporous mysteries no more.

Owing to the unhappy subdivisions of our studies, such phenomena as these—constant companions of the breeder—come seldom within the purview of modern science, which, forced for a moment to contemplate them, expresses astonishment and relapses into indolent scepticism. It is in the hope that a little may be done to draw research back into these forgotten paths that I avail myself of this great opportunity of speaking to my colleagues with somewhat wider range of topic than is possible within the limits of a scientific paper. For I am convinced that the investigation of heredity by experimental methods offers the sole chance of progress with the fundamental problems of evolution.

In saying this I mean no disrespect to that study of the physiology of reproduction by histological means, which, largely through the stimulus of Weismann's speculations, has of late made such extraordinary advances. It needs no penetration to see that, by an exact knowledge of the processes of maturation and fertilization, a vigorous stock is being reared, upon which some day the experience of the breeder will be firmly grafted, to our mutual profit. We, who are engaged in experimental breeding, are watching with keenest interest the researches of Strasburger, Boveri, Wilson, Farmer and their many fellow-workers and associates in this difficult field, sure that in the near future we shall be operating in common. We know already that the experience of the breeder is in no way opposed to the facts of the histologist; but the point at which we shall unite will be found when it is possible to trace in the maturing germ an indication of some character afterwards recognizable in the resulting organism. Till then, in order to pursue directly the course of heredity and variation, it is evident that we must

fall back on those tangible manifestations which are to be studied only by field observation and experimental breeding.

The breeding-pen is to us what the test-tube is to the chemist—an instrument whereby we examine the nature of our organisms and determine empirically what for brevity I may call their genetic properties. As unorganized substances have their definite properties, so have the several species and varieties which form the materials of our experiments. Every attempt to determine these definite properties contributes immediately to the solution of that problem of problems, the physical constitution of a living organism. In those morphological studies which I suppose most of us have in our time pursued, we sought inspiration from the belief that in the examination of present normalities we were tracing the past, the phylogenetic order of our types, the history—as we conceived—of evolution. In the work which I am now pressing upon your notice we may claim to be dealing not only with the present and the past, but with the future also.

On such an occasion as this it is impossible to present to you in detail the experiments—some exceedingly complex—already made in response to this newer inspiration. I must speak of results, not of methods. At a later meeting, moreover, there will be opportunities of exhibiting practically to those interested some of the more palpable illustrations. It is also impossible to-day to make use of the symbolic demonstrations by which the lines of analysis must be represented. The time can not be far distant when ordinary Mendelian formulæ will be mere *as in præsenti* to a biological audience. Nearly five years have passed since this extraordinary rediscovery was made known to the scientific world by the practically simultaneous papers of De Vries, Correns and Tschermak, not to speak of thirty-five years of neglect endured before. Yet a phenomenon comparable in significance with any that biological science has revealed remains the intellectual possession of specialists. We still speak sometimes of Mendel's hypothesis or theory, but in truth the terms have no strict application. It is no theory that water is made up of hydrogen and oxygen, though we can not watch the atoms unite, and it is no theory that the blue Andalusian fowl I produce was made by the meeting of germ-cells bearing respectively black and a peculiar white. Both are incontrovertible facts deduced from observation. The two facts have this in common also, that their perception gives us a glimpse into that hidden order out of which the seeming disorder of our world is built. If I refer to Mendelian 'theory,' therefore, in the words with which Bacon introduced his Great Instauration, 'I entreat men to believe that it is not an opinion to be held, but a work to be done; and to be well assured that I am laboring to lay the foundation, not of any sect or doctrine, but of human utility and power.'

ON THE PERCEPTION OF THE FORCE OF GRAVITY BY PLANTS.

By FRANCIS DARWIN, F.R.S., Fellow of Christ College,
President of the Botanical Section of the British Association.

WHEN I had the honor of addressing this association at Cardiff as president of the mother section from which ours has sprung by fission—I spoke of the mechanism of the curvatures commonly known as tropisms. To-day I propose to summarize the evidence—still far from complete—which may help us to form a conception of the mechanism of the stimulus which calls forth one of these movements—namely, geotropism. I have said that the evidence is incomplete, and perhaps I owe you an apology for devoting the time of this section to an unsolved problem. But the making of theories is the romance of research; and I may say, in the words of Diana of the Crossways, who indeed spoke of romance, 'The young who avoid that region escape the title of fool at the cost of a celestial crown.' I am prepared for the risk in the hope that in not avoiding the region of hypothesis I shall at least be able to interest my hearers.

The modern idea of the behavior of plants to their environment has been the growth of the last twenty-five years; though, as Pfeffer has shown, it was clearly stated in 1824 by Dutrochet, who conceived the movements of plants to be 'spontaneous'—*i. e.*, to be executed at the suggestion of changes in the environment, not as the direct and necessary result of such changes. I have been in the habit of expressing the same thought in other words, using the idea of a guide or signal, by the interpretation of which plants are able to make their way successfully through the difficulties of their surroundings. In the existence of the force of gravity we have one of the most striking features of the environment, and in the sensitiveness to gravity wh ch exists in plants we have one of the most widespread cases of a plant reading a signal and directing its growth in relation to its perception. I use the word perception not of course to imply consciousness, but as a convenient form of expression for a form of irritability. It is as though the plant discovered from its sensitiveness to gravity the line of the earth's radius, and then chose a line of growth bearing a certain relation to the vertical line so discovered, either parallel to it or across it at various angles. This, the reaction or reply to the stimulus, is, in my judgment, an adaptive act forced on the species by the struggle for life. This point of view, which, as I regret to think, is not very fash-

ionable, need not trouble us. We are not concerned with why the plant grows up into the air or down into the ground; we are only concerned with the question of how the plant perceives the existence of gravitation. Or, in other words, taking the reaction for granted, what is the nature of the stimulus? If a plant is beaten down by wind or by other causes into a horizontal position, what stimulative change is wrought in the body of the plant by this new posture?

It is conceivable in the case of a stem supported by one end and projecting freely in the air that the unaccustomed state of strain might act as a signal. The tissues on one side (the upper) are stretched, and they are compressed below; this might guide the plant; it might, in fact, have evolved the habit of rapid growth in the compressed side. This is only given as an illustration, for we know that the stimulus does not arise in this way, since such a plant, supported throughout its length, and, therefore, suffering no strain, is geotropically stimulated. The illustration is so far valuable, as it postulates a stimulus produced by weight, and we know from Knight's centrifugal experiment that weight is the governing factor in the conditions. Since we can not believe that the stimulus arises from the strain as affecting the geotropic organ as a whole, we must seek for weight-effects in the individual cell of which the plant is built. We must, in fact, seek for weight-effects on the ectoplasm of those cells which are sensitive to the stimulus of gravity.

If we imagine a plant consisting of a single apogeotropic cell we shall see that the hydrostatic pressure of the cell-contents might serve as a signal.

As long as the cell is vertical the hydrostatic pressure of the cell-sap upon the ectoplasm at *C* (Fig. 1) is equal to that at *D*. But the pressure on the basal wall, *B*, differs from that at *A* (the apical wall)

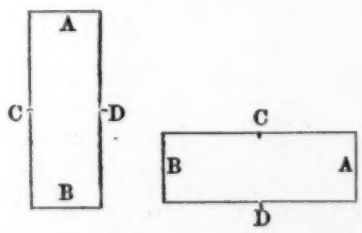


FIG. 1.

by the weight of the column *AB*. If the plant be forced into the horizontal, the pressure at *A* and *B* becomes the same, while the pressure at *C* no longer equals that at *D*, but differs by the weight of the column *CD*. Here undoubtedly is a possible means by which the plant could perceive that it was no longer vertical, and would have the means of

distinguishing up from down. So that if it were an apogeotropic plant it would need to develop the instinct of relatively accelerated growth on the side *D*, on which the pressure is greatest.

What is here roughly sketched is the groundwork of the theory of *graviperception* suggested by Pfeffer and supported by Czapek, which I shall speak of as the radial pressure theory, and to which I shall return later.

It is obvious that there is another consideration to be taken into account, namely, that cells do not contain cell-sap only, but various bodies—nucleus, chloroplasts, crystals, etc.—and that these bodies, differing in specific gravity from the cell-sap, will exert pressure on the physically lower or physically higher cell-walls according as they are heavier or lighter than the cell-sap. Here we have the possibility of a sense-organ for verticality. As long as the stem is vertical and the apex upwards the heavy bodies rest on the basal wall, and the plant is not stimulated to curvature; but if placed horizontally, so that the heavy bodies rest on the lateral cell-walls, which are now horizontal, the plant *is* stimulated to curve. This is known as the *statolith theory*.

It seems to me quite certain that the stimulus must originate either in the weight of solid particles or in the weight of the fluid in the cells, or by both these means together. And for this reason. Take the statolith theory first. There undoubtedly are heavy bodies in cells; for instance, certain loose, movable starch-grains. Now, either these starch-grains are specialized to serve the purpose of graviperception or they are not. If they are so specialized, *cadit quæstio;* if they are not, there still remains this interesting point of view; the starch-grains fall to the lower end of the cells in which they occur; therefore, shortly before every geotropic curvature which has taken place since movable starch-grains came into existence, there has been a striking change in the position of these heavy cell contents. Now, if we think of the evolution of geotropism as an adaptive manner of growth we must conceive plants growing vertically upwards and succeeding in life, others not so behaving, and consequently failing. There will be a severe struggle tending to pick out those plants which associated certain curvatures with certain preceding changes, and therefore it seems to me that, if movable starch-grains were originally in no way specialized as part of the machinery of graviperception, they would necessarily become an integral part of that machinery, since the act of geotropism would become adherent to or associated with the falling of the starch-grains.

This argument must in fairness be applied to any other physical conditions which constantly precede geotropic curvature; it is, therefore, not an argument in favor of the statolith theory alone, but equally

for the pressure theory, and can not help us to decide between the two points of view.

Are there any general considerations which can help us to decide for or against the statolith theory? I think there are—namely, (1) analogy with the graviperceptive organs of animals; (2) the specialization and distribution of the falling bodies in plants.

Berthold (to whom the credit is due of having first suggested that Dehnecke's falling starch-grains might function as originators of geotropic reaction) is perhaps somewhat bold in saying that 'the primary effect of gravity' as regards stimulation must depend on the passive sinking of the heavier parts. Noll, too, says that Knight's experiment depends on weight, and not the weight of complete parts of the plant-body, but of weight within the irritable structure. I can not see that these downright statements are justified on direct evidence, and I accordingly lay some stress on the support of zoological evidence. It has been conclusively proved by Kreidl's beautiful experiment that in the Crustacean *Palæmon* the sense of verticality depends on the pressure of heavy bodies on the inside of cavities now known as statocysts, and formerly believed to be organs of hearing. The point of the experiment is that when the normal particles are replaced by fragments of iron the *Palæmon* reacts towards the attraction of a magnet precisely as it formerly reached towards gravity.

It is unfortunate that Noll's arguments in favor of the existence of a similar mechanism in plants were not at once followed by the demonstration of those easily visible falling bodies, which, in imitation more flattering than accurate, are called *statoliths,* after the bodies in the statocysts of animals. Personally I was convinced by Kreidl, as quoted by Noll, that here was the key to graviperception in plants. But it was not until the simultaneous appearance of Haberlandt's and Němec's papers that my belief became active, and this, I think, was the case with others. The whole incident is an instance of what my father says somewhere about the difficulty of analyzing the act of belief. I find it impossible to help believing in the statolith theory, though I own to not being able to give a good account of the faith that is in me. It is a fair question whether the analogy drawn from animals gives any support to the theory for plants. The study of sense-organs in plants dates, I think, in its modern development, at least, from my father's work on root-tips, and on the light-perceiving apices of certain seedlings. And the work on the subject is all part of the wave of investigation into adaptations which followed the publication of the 'Origin of Species.' It is very appropriate that one of the two authors to whom we owe the practical working out of the statolith theory should also be one of the greatest living authorities on adaptation in plants. Haberlandt's work on sense-organs, especially on the apparatus for the

reception of contact stimuli, is applicable to our present case, since he has shown that the organs for intensifying the effect of contact are similar in the two kingdoms. No one supposes that the whisker of a cat and the sensitive papilla of a plant are phylogenetically connected. It is a case of what Ray Lankester called homoplastic resemblance. Necessity is the mother of invention, but invention is not infinitely varied, and the same need has led to similar apparatus in beings which have little more in common than that both are living organisms.

But, whether we are or are not affected in our belief by the general argument from analogy, we can not neglect the important fact that Kreidl proves the possibility of gravisensitiveness depending on the possession of statoliths. We must add to this a very important consideration—namely, that we know from Němec's work that an alteration in the position of the statoliths does stimulate the *statocyte*. Such, at least, is, to my mind, the only conclusion to be drawn from the remarkable accumulation of protoplasm which occurs, for instance, on the basal wall of a normally vertical cell when that wall is cleared of statoliths by temporary horizontality. The fact that a visible disturbance in the plasmic contents of the statocyte follows the disturbance of the starch-grains seems to me a valuable contribution to the evidence.

There is one other set of facts of sufficiently general interest to find a place in this section. I mean Haberlandt's result, also independently arrived at by myself, that when a plant is placed horizontally and rapidly shaken up and down in a vertical plane the gravistimulus is increased. This is readily comprehensible on the statolith theory, since we can imagine the starch-grains would give a greater stimulus if made to vibrate on one of the lateral walls, or if forced into the protoplasm, as Haberlandt supposes. I do not see that the difference in the pressure of the cell-sap on the upper and lower walls (*i. e.*, the lateral walls morphologically considered) would be increased. It would, I imagine, be rendered uneven; but the average difference would remain the same. But in the case of the starch-grains an obvious new feature is introduced by exchanging a stationary condition for one of movement. And though I speak with hesitation on such a point, I am inclined to see in Haberlandt's and my own experiments a means of distinguishing between the pressure and statolith theories. Noll, however, considers that the shaking method is not essentially different from that of Knight's experiment, and adds that the result might have been foreseen.

THE ETHNOLOGICAL WORK OF LANE FOX.

By HENRY BALFOUR, M.A.,

PRESIDENT OF THE ANTHROPOLOGICAL SECTION OF THE BRITISH ASSOCIATION.

THE earth, as we know, is peopled with races of the most heterogeneous description, races in all stages of culture. Colonel Lane Fox argued that, making due allowance for possible instances of degradation from a higher condition, this heterogeneity could readily be explained by assuming that, while the progress of some races has received relatively little check, the culture development of other races has been retarded to a greater or less extent, and that we may see represented conditions of at least partially arrested development. In other words, he considered that in the various manifestations of culture among the less civilized peoples were to be seen more or less direct *survivals* from the earlier stages or strata of human evolution; vestiges of ancient conditions which have fallen out at different points and have been left behind in the general march of progress.

Taken together, the various living races of man seem almost to form a kind of living genealogical tree, as it were, and it is as an epiphyte upon this tree that the comparative ethnologist largely thrives; while to the archeologist it may also prove a tree of knowledge the fruit of which may be eaten with benefit rather than risk.

This certainly seems to be a legitimate assumption in a general way; but there are numerous factors which should be borne in mind when we endeavor to elucidate the past by means of the present. If the various gradations of culture exhibited by the condition of living races—the savage, semi-civilized, or barbaric, and the civilized races—could be regarded as accurately typifying the successive stages through which the higher forms of culture have been evolved in the course of the ages; if, in fact, the different modern races of mankind might be accepted as so many sections of the human race whose intellectual development has been arrested or retarded at various definite stages in the general progression, then we should have, to all intents and purposes, our genealogical tree in a very perfect state, and by its means we could reconstruct the past and study with ease the steady growth of culture and handicrafts from the earliest simple germs, reflecting the mental condition of primeval man up to the highest manifestations of the most cultured races.

These ideal conditions are, however, far from being realized. Intellectual progress has not advanced along a single line, but, in its

development, it has branched off in various directions, in accordance with varying environment; and the tracing of lines of connection between different forms of culture, as is the case with the physical variations, is a matter of intricate complexity. Migrations with the attendant climatic changes, change of food, and, in fact, of general environment, to say nothing of the crossing of different stocks, transmission of ideas from one people to another, and other factors, all tend to increase the tangle.

Although in certain instances savage tribes or races show obvious signs of having *degenerated* to some extent from conditions of a higher culturedom, this can not be regarded as the general rule, and we must always bear in mind the seemingly paradoxical truth that degradation in the culture of the lower races is often, if not usually, the direct result of contact with peoples in a far higher state of civilization.

There can, I think, be little doubt that Colonel Lane Fox was well justified in urging the view that most savage races are in large measure strictly *primitive,* survivals from early conditions, the development of their ideas having from various causes remained practically stationary during a very considerable period of time. In the lower, though not degenerate, races signs of this are not wanting, and while few, possibly none, can be said to be absolutely in a condition of arrested development, their normal progress is at a slow, in most cases at a *very* slow, rate.

Perhaps the best example of a truly primitive race existing in recent times, of which we have any knowledge, was afforded by the native inhabitants of Tasmania. This race was still existing fifty years ago, and a few pure-blooded survivors remained as late as about the year 1870, when the race became extinct, the benign civilizing influence of enlightened Europeans having wiped this extremely interesting people off the face of the earth. The Australians, whom Colonel Lane Fox referred to as being 'the lowest amongst the existing races of the world of whom we have any accurate knowledge,' are very far in advance of the Tasmanians, whose lowly state of culture conformed thoroughly with the characteristics of a truly primitive race, a survival not only from the stone age in general, but from almost the earliest beginnings of the stone age. The difference between the culture of the Tasmanians and that of the Australians was far greater than that which exists between man of the 'river drift' period and his Neolithic successors. The objects of every-day use were but slight modifications of forms suggested by nature, involving the exercise of merely the simplest mental processes. The stone implements were of the rudest manufacture, far inferior in workmanship to those made by Paleolithic man; they were never ground or polished, never even fitted with handles, but were merely grasped in the hand. The *varieties* of imple-

ments were very *few in number,* each, no doubt, serving a number of purposes, the function varying with the requirements of the moment. They had no bows or other appliances for accelerating the flight of missiles, no pottery, no permanent dwellings; nor is there any evidence of a previous knowledge of such products of higher culture. They seem to represent a race which was isolated very early from contact with higher races; in fact, before they had developed more than the merest rudiments of culture—a race continuing to live under the most primitive conditions, from which they were never destined to emerge.

Between the Tasmanians, representing in their very low culture the one extreme, and the most civilized peoples at the other extreme, lie races exhibiting in a general way intermediate conditions of advancement or retardation. If we are justified, as I think we are, in regarding the various grades of culture observable among the more lowly of the still existing races of man as representing to a considerable extent those vanished cultures which in their succession formed the different stages by which civilization emerged gradually from a low state, it surely becomes a very important duty for us to study with energy these living illustrations of early human history in order that the archeological record may be supplemented and rendered more complete. The material for this study is vanishing so fast with the spread of civilization that opportunities lost now will never be regained, and already even it is practically impossible to find native tribes which are wholly uncontaminated with the products, good or bad, of higher cultures.

The arts of living races help to elucidate what is obscure in those of prehistoric times by the process of reasoning from the known to the unknown. It is the work of the zoologist which enables the paleontologist to reconstruct the forms of extinct animals from such fragmentary remains as have been preserved, and it is largely from the results of a comparative study of living forms and their habitats that he is able, in his descriptions, to equip the reconstructed types of a past fauna with environments suited to their structure, and to render more complete the picture of their mode of life.

In like manner, the work of the ethnologist can throw light upon the researches of the archeologist; through it broken sequences may be repaired, at least suggestively, and the interpretation of the true nature and use of objects of antiquity may frequently be rendered more sure. Colonel Lane Fox strongly advocated the application of the reasoning methods of biology to the study of the origin, phylogeny and etionomics of the arts of mankind, and his own collection demonstrated that the products of human intelligence can conveniently be classified into families, genera, species and varieties, and *must* be so grouped if their affinities and development are to be investigated.

It must not be supposed—although some people, through misap-

prehension of his methods, jumped at this erroneous conclusion—that he was unaware of the danger of possibly mistaking mere accidental resemblances for morphological affinities, and that he assumed that *because* two objects, perhaps from widely separated regions, appeared more or less identical in form, and possibly in use, they were necessarily to be considered as members of one phylogenetic group. On the contrary, in the grouping of his specimens according to their form and function, he was anxious to assist as far as possible in throwing light upon the question of the monogenesis or polygenesis of certain arts and appliances, and to discover whether they are exotic or indigenous in the regions in which they are now found, and, in fact, to distinguish between mere analogies and true homologies. If we accept the theory of the monogenesis of the human race, as most of us undoubtedly do, we must be prepared to admit that there prevails a condition of unity in the tendencies of the human mind to respond in a similar manner to similar stimuli. Like conditions beget like results; and thus instances of independent invention of similar objects are liable to arise. For this very reason, however, the arts and customs belonging to even widely separated peoples may, though apparently unrelated, help to elucidate some of the points in each other's history which remain obscure through lack of the evidence required to establish *local* continuity.

I think, moreover, that it will generally be allowed that cases of 'independent invention' of similar forms should be considered to have established their claim to be regarded as such only after exhaustive inquiry has been made into the possibilities of the resemblances being due to actual relationship. There is the alternative method of assuming that, because two like objects are widely separated geographically, and because a line of connection is not immediately obvious, therefore the resemblance existing between them is fortuitous, or merely the natural result of similar forms having been produced to meet similar needs. Premature conclusions in matters of this kind, though temptingly easy to form, are not in the true scientific spirit, and act as a check upon careful research, which, by investigating the case in its various possible aspects, is able either to prove or disprove what otherwise would be merely a hasty assumption. The association of similar forms into the same series has therefore a double significance. On the one hand, the sequence of related forms is brought out, and their geographical distribution illustrated, throwing light, not only upon the evolution of types, but also upon the interchange of ideas by transferrence from one people to another, and even upon the migration of races. On the other hand, instances in which two or more peoples have arrived independently at similar results are brought prominently forward, not merely as interesting coincidences, but also as evidence pointing to the phylogenetic

unity of the human species, as exemplified by the tendency of human intelligence to evolve independently identical ideas where the conditions are themselves identical. Polygenesis in his inventions may probably be regarded as testimony in favor of the monogenesis of man.

I have endeavored in this address to dwell upon some of the main principles laid down by Colonel Lane Fox as a result of his special researches in the field of ethnology, and my object has been twofold. First, to bear witness to the very great importance of his contribution to the scientific study of the arts of mankind and the development of culture in general, and to remind students of anthropology of the debt which we owe to him, not only for the results of his very able investigations, but also for the stimulus which he imparted to research in some of the branches of this comprehensive science. Secondly, my object has been to reply to some criticisms offered in regard to points in the system of classification adopted in arranging his ethnographical collection. And, since such criticisms as have reached me have appeared to me to be founded mainly upon misinterpretation of this system, I have thought that I could meet them best by some sort of restatement of the principles involved.

It would be unreasonable to expect that his work should hold good in all details. The early illustrations of his theories were to be regarded as tentative rather than dogmatic, and in later life he recognized that many modifications in matters of detail were rendered necessary by new facts which had since come to light. The crystallization of solid facts out of a matrix which is necessarily partially volatile is a process requiring time. These minor errors and the fact of our not agreeing with all his details in no way invalidate the general principles which he urged, and we need but cast a cursory glance over recent ethnological literature to see how widely accepted these general principles are, and how they have formed the basis of, and furnished the inspiration for, a vast mass of research by ethnologists of all nations.

It appears more than probable that Cambridge will be much involved in the future advancement of anthropological studies in Great Britain, if we may judge from the evident signs of a growing interest in the science, not the least of which is the recent establishment of a board of anthropological studies, an important development upon which we may well congratulate the university. Within my own experience there have been many proofs of the existence in Cambridge of a keen sympathy with the principles of ethnological inquiry developed by Colonel Lane Fox, and I feel that, as regards my choice of a theme for the main topic of my address, no apology is needed. For my handling of this theme, on the other hand, I fear it must be otherwise. I would gladly have done fuller justice to the work of Colonel Lane Fox, but, while I claim to be among the keenest of his disciples, I must confess to being but an indifferent apostle.

I have been obliged, moreover, to pass over many interesting features in the work of this ingenious and versatile scientist. I have made no attempt to touch upon his archeological researches, since it has been necessary for me to restrict myself to a portion only of his scientific work. In this field, as in his ethnological work, his keen insight, ingenuity and versatility were manifested, while the close attention which he bestowed upon matters of minute detail has rendered classical his work as a field archeologist. While the greater part of his ethnological work is associated with the name Lane Fox, by which he was known until 1880, most of his researches into the remains of prehistoric times were conducted after he had in that year assumed the name of Pitt Rivers, on inheriting an important estate which, by the happiest of coincidences, included within its boundaries a considerable number of prehistoric sites of the highest importance. That he made full use of his opportunities is amply manifested in his published works. In his archeological work are repeated the characteristics of his ethnological researches, and one may with confidence say of his contributions to both fields of inquiry that, if he advanced science greatly through his results he furthered its progress even more through his methods. By his actual achievements as a researcher he pushed forward the base of operations; by his carefully-thought-out systems for directing research he developed a sound strategical policy upon which to base further organized attacks upon the unknown.

ON MOUNTAINS AND MANKIND.

By DOUGLAS W. FRESHFIELD,
PRESIDENT OF THE GEOGRAPHICAL SECTION OF THE BRITISH ASSOCIATION.

WE have all of us seen hills, or what we call hills, from the monstrous protuberances of the Andes and the Himalaya to such puny pimples as lie about the edges of your fens. Next to a waterfall, the first natural object (according to my own experience) to impress itself on a child's mind is a hill, some spot from which he can enlarge his horizon. Hills, and still more mountains, attract the human imagination and curiosity. The child soon asks, 'Tell me, how were mountains made?' a question easier to ask than to answer, which occupied the lifetime of the father of mountain science, De Saussure. But there are mountains and mountains. Of all natural objects the most impressive is a vast snowy peak rising as a white island above the waves of green hills—a fragment of the arctic world left behind to commemorate its past predominance—and bearing on its broad shoulders a garland of the Alpine flora that has been destroyed on the lower ground by the rising tide of heat and drought that succeeded the last glacial epoch. Mid-summer snows, whether seen from the slopes of the Jura or the plains of Lombardy, above the waves of the Euxine or through the glades of the tropical forests of Sikhim, stir men's imaginations and rouse their curiosity. Before, however, we turn to consider some of the physical aspects of mountains, I shall venture, speaking as I am here to a literary audience, and in a university town, to dwell for a few minutes on their place in literature—in the mirror that reflects in turn the mind of the passing ages. For geography is concerned with the interaction between man and nature in its widest sense. There has been recently a good deal of writing on this subject—I can not say of discussion, for of late years writers have generally taken the same view. That view is that the love of mountains is an invention of the nineteenth century, and that in previous ages they had been generally looked on either with indifference or positive dislike, rising in some instances to abhorrence. Extreme examples have been repeatedly quoted. We have all heard of the bishop who thought the devil was allowed to put in mountains after the fall of man; of the English scribe in the tenth century who invoked 'the bitter blasts of glaciers and the Pennine host of demons' on the violaters of the charters he was employed to draft. The examples on the other side have been comparatively neglected. It seems time they were insisted on.

The view I hold firmly, and which I wish to place before you today, is that this popular belief that the love of mountains is a taste, or, as some would say, a mania, of advanced civilization, is erroneous. On the contrary, I allege it to be a healthy, primitive and almost universal human instinct. I think I can indicate how and why the opposite belief has been fostered by eminent writers. They have taken too narrow a time limit for their investigation. They have compared the nineteenth century not with the preceding ages, but with the eighteenth. They have also taken too narrow a space limit. They have hardly cast their eyes beyond western Europe. Within their own limits I agree with them. The eighteenth century was, as we all know, an age of formality. It was the age of Palladian porticoes, of interminable avenues, of formal gardens and formal style in art, in literature and in dress. Mountains, which are essentially romantic and Gothic, were naturally distasteful to it. The artist says 'they will not compose,' and they became obnoxious to a generation that adored composition, that thought more of the cleverness of the artist than of the aspects of nature he used as the material of his work. There is a great deal to be said for the century; it produced some admirable results. It was a contented and material century, little stirred by enthusiasms and aspirations and vague desires. It was a phase in human progress, but in many respects it was rather a reaction than a development from what had gone before. Sentiment and taste have their tides like the sea, or, we may here perhaps more appropriately say, their oscillations like the glaciers. The imagination of primitive man abhors a void, it peoples the regions it finds uninhabitable with aery sprites, with 'Pan and father Sylvanus and the sister nymphs,' it worships on high places and reveres them as the abode of deity. Christianity came and denounced the vague symbolism and personification of nature in which the pagan had recognized and worshipped the unseen. It found the objects of its devotion not in the external world, but in the highest moral qualities of man. Delphi heard the cry 'Great Pan is dead!' But the voice was false. Pan is immortal. Every villager justifies etymology by remaining more or less of a pagan. Other than villagers have done the same. The monk driven out of the world by its wickedness fell in love with the wilderness in which he sought refuge, and soon learned to give practical proof of his love of scenery by his choice of sites for his religious houses. But the literature of the eighteenth century was not written by monks or countrymen, or by men of world-wide curiosity and adventure like the Italians of the renaissance or our Elizabethans. It was the product of a practical common-sense epoch which looked on all waste places, heaths like Hindhead, or hills like the Highlands, as blemishes in the scheme of the universe, not having yet recognized their final purpose as golf

links or gymnasiums. Intellectual life was concentrated in cities and courts, it despised the country. Books were written by townsmen, dwellers in towns which had not grown into vast cities, and whose denizens therefore had not the longing to escape from their homes into purer air that we have to-day. They abused the Alps frankly. But all they saw of them was the comparatively dull carriage passes, and these they saw at the worst time of year. Hastening to Rome for Easter, they traversed the Maurienne while the ground was still brown with frost and patched untidily with half-melted snowdrifts. It is no wonder that Gray and Richardson, having left spring in the meadows and orchards of Chambéry, grumbled at the wintry aspect of Lanslebourg.

That at the end of the eighteenth century a literary lady of western Europe preferred a Paris gutter to the Lake of Geneva is an amusing caricature of the spirit of the age that was passing away, but it is no proof that the love of mountains is a new mania, and that all earlier ages and peoples looked on them with indifference or dislike. Wordsworth and Byron and Scott in this country, Rousseau and Goethe, De Saussure and his school abroad broke the ice, but it was the ice of a winter frost, not of a glacial period.

Consider for a moment the literature of the two peoples who have most influenced European thought—the Jews and the Greeks. I need hardly quote a book that before people quarrelled over education was known to every child—the Bible. I would rather refer you to a delightful poem in rhyming German verse written in the seventeenth century by a Swiss author, Rebman, in which he relates all the great things that happened on mountains in Jewish history: how Solomon enjoyed his Sommerfrische on Lebanon, and Moses and Elias both disappeared on mountain tops; how kings and prophets found their help among the hills; how closely the hills of Palestine are connected with the story of the Gospels.

Consider, again, Greece, where I have just been wandering. Did the Greeks pay no regard to their mountains? They seized eagerly on any striking piece of hill scenery and connected it with a legend or a shrine. They took their highest mountain, broad-backed Olympus, for the home of the gods; their most conspicuous mountain, Parnassus, for the home of poetry. They found in the cliffs of Delphi a dwelling for their greatest oracle and a center for their patriotism. One who has lately stood on the top of Parnassus and seen the first rays of the sun as it springs from the waves of the Ægean strike its snows, while Attica and Bœotia and Eubœa still lay in deep shadow under his feet, will appreciate the famous lines of Sophocles, which I will not quote, as I am uncertain how you may pronounce Greek in this university. You may remember, too, that Lucian makes Hermes take Charon, when he

has a day out from Hell, to the twin-crested summit, and show him the panorama of land and sea, of rivers and famous cities. The Vale of Tempe, the deep gap between Olympus and Ossa, beautiful in its great red cliffs, fountains and spreading plane-trees, was part of a Roman's classical tour. The superb buttresses in which Taygetus breaks down on the valley of the Eurotas were used by the Spartans for other purposes besides the disposal of criminals and weakly babies. The middle regions—the lawns above the Langada Pass, 'virginibus bacchata Lacænis Taygeta'—are frequented to this day as a summer resort by Spartan damsels. The very top, the great rock that from a height of 8,000 feet looks down through its woods of oaks and Aleppo pines on the twin bays of the southern sea, is a place of immemorial pilgrimages. It is now occupied by a chapel framed in a tiny court, so choked with snow at the beginning of June that I took the ridge of the chapel roof for a dilapidated stoneman. I have no time to-day to look for evidence in classical literature, to refer to the discriminating epithets applied in it to mountain scenes.

A third race destined apparently to play a great part in the world's history—the Japanese—are ancient mountain lovers. We are all aware that Fusiyama to the Japanese is (as Ararat to the Armenians) a national symbol; that its ascent is constantly made by bands of pilgrims; that it is depicted in every aspect. Those who have read the pleasant book of Mr. Watson, who, as English chaplain for some years at Tokio, had exceptional opportunities of travel in the interior, will remember how often he met with shrines and temples on the summits of the mountains, and how he found pilgrims who frequented them in the belief that they fell there more readily into spiritual trances. The Japanese Minister, when he attended Mr. Watson's lecture at the Alpine Club, told us that his countrymen never climbed mountains without a serious—that is to say, a religious—object.

India and China would add to my evidence had I knowledge and time enough to refer to their literature. I remember Tennyson pointing out to me in a volume of translations from the Chinese a poem, written about the date of King Alfred, in praise of a picture of a mountain landscape. But I must return to the sixteenth and seventeenth centuries in Europe; I may go earlier—even back to Dante. His allusions to mountain scenery are frequent; his Virgil had all the craft of an Alpine rock-climber. Read Leonardo da Vinci's 'Notes,' Conrad Gesner's 'Ascent of Pilatus'; study the narratives of the Alpine precursors Mr. Coolidge has collected and annotated with admirable industry in the prodigious volume he has recently brought out.

It is impossible for me here to multiply proofs of my argument, to quote even a selection from the passages that show an authentic enthusiasm for mountains that may be culled from writers of various

nations prior to A. D. 1600. I must content myself with the following specimens, which will probably be new to most of my hearers.

Benoît Marti was a professor of Greek and Hebrew at Bern, and a friend of the great Conrad Gesner (I call him great, for he combined the qualities of a man of science and a man of letters, was one of the fathers of botany as well as of mountaineering, and was, in his many-sidedness, a typical figure of the renaissance). Marti, in the year 1558 or 1559, wrote as follows of the view from his native city:

"These are the mountains which form our pleasure and delight" (the Latin is better—'deliciæ nostræ, nostrique amores') "when we gaze at them from the higher parts of our city and admire their mighty peaks and broken crags that threaten to fall at any moment. Here we watch the risings and settings of the sun and seek signs of the weather. In them we find food not only for our eyes and our minds but also for our bellies"; and he goes on to enumerate the dairy products of the Oberland and the happy life of its population. I quote again this good man: "Who, then, would not admire, love, willingly visit, explore, and climb places of this sort? I assuredly should call those who are not attracted by them mushrooms, stupid, dull fishes, and slow tortoises" ('fungos, stupidos insulsos pisces, lentosque chelones'). "In truth, I cannot describe the sort of affection and natural love with which I am drawn to mountains, so that I am never happier than on the mountain crests, and there are no wanderings dearer to me than those on the mountains. They are the theater of the Lord, displaying monuments of past ages, such as precipices, rocks, peaks and chasms, and never-melting glaciers"; and so on through many eloquent paragraphs.

I will only add two sentences from the preface to Simler's 'Vallesiæ et Alpium Descriptio,' first published in 1574, which seems to me a strong piece of evidence in favor of my view: "In the entire district, and particularly in the very lofty ranges by which the Vallais is on all sides surrounded, wonders of nature offer themselves to our view and admiration. With my countrymen many of them have through familiarity lost their attraction; but foreigners are overcome at the mere sight of the Alps, and regard as marvels what we through habit pay no attention to."

Mr. Coolidge, in his singularly interesting footnotes, goes on to show that the books that remain to us are not isolated instances of a feeling for mountains in the age of the renaissance. The mountains themselves bear, or once bore, records even more impressive. Most of us have climbed to the picturesque old castle at Thun and seen beyond the rushing Aar the green heights of the outposts of the Alps, the Stockhorn and the Niesen. Our friend, Marti, who climbed the former peak about 1558, records that he found on the summit 'tituli, rythmi, et proverbia saxis inscripta unà cum imaginibus et nominibus

auctorum. Inter alia cujusdam docti et montium amœnitate capti observare licebat illud:

'Ο τῶν ὀρων ἔρως αριστος.'

'The love of mountains is best.' In those five words some Swiss professor anticipated the doctrine of Ruskin and the creed of Leslie Stephen, and of all men who have found mountains the best companions in the vicissitudes of life.

In the annals of art it would be easy to find additional proof of the attention paid by men to mountains three to four hundred years ago. The late Josiah Gilbert, in a charming but too little-known volume, 'Landscape in Art,' has shown how many great painters depicted in their backgrounds their native hills. Titian is the most conspicuous example.

It will perhaps be answered that this love of mountains led to no practical result, bore no visible fruit, and therefore can have been but a sickly plant. Some of my hearers may feel inclined to point out that it was left to the latter half of the nineteenth century to found climbers' clubs. It would take too long to adduce all the practical reasons which delayed the appearance of these fine fruits of peace and an advanced civilization. I am content to remind you that the love of mountains and the desire to climb them are distinct tastes. They are often united, but their union is accidental not essential. A passion for golf does not necessarily argue a love of levels. I would suggest that more outward and visible signs than is generally imagined of the familiar relations between men and mountains in early times may be found. The choicest spots in the Alpine region—Chamonix, Engelberg, Disentis, Einsiedlen, Pesio, the Grande Chartreuse—were seized on by recluses; the Alpine baths were in full swing at quite an early date. I will not count the Swiss Baden, of which a geographer, who was also a Pope, Æneas Silvius (Pius II.) records the attractions, for it is in the Jura, not the Alps; but Pfäfers, where wounded warriors went to be healed, was a scene of dissipation, and the waters of St. Moritz were vaunted as superseding wine. I may be excused, since I wrote this particular passage myself a good many years ago, for quoting a few sentences bearing on this point from 'Murray's Handbook to Switzerland.' In the sixteenth century fifty treatises dealing with twenty-one different resorts were published. St. Moritz, which had been brought into notice by Paracelsus (died 1541), was one of the most famous baths. In 1501 Matthew Schinner, the famous Prince Bishop of Sion, built 'a magnificent hotel' at Leukerbad, to which the wealthy were carried up in panniers on the back of mules. Brieg, Gurnigel, near Bern, the baths of Masino, Tarasp and Pfäfers were also popular in early times. Leonardo da Vinci mentions the baths of Bormio, and Gesner went there.

CORRELATION OF REFLEXES AND THE PRINCIPLE OF THE COMMON PATH.

By Professor C. S. SHERRINGTON, M.A., D.Sc., M.D., LL.D., F.R.S.,
President of the Physiological Section of the British Association.

PHYSIOLOGY studies the nervous system from three main points of view. One of these regards its processes of nutrition. Nerve-cells, as all cells, lead individual lives, breathe, dispense their own stores of energy, repair their own substantial waste, are, in short, living units, each with a nutrition more or less centered in itself. The problems of nutrition of the nerve-cell and of the nervous system, though partly special to this specially differentiated form of cell life, are, on the whole, accessible to the same methods as is nutrition in other cells and in the body as a whole.

But besides the essential functions common to all living cells, the cells of the nervous system present certain which are specialized. Among properties of living matter, one by its high development in the nerve-cell may be said to characterize it. I mean the cell's transmission of excitement spatially along itself and thence to other cells. This 'conductivity' is the specific physiological property of nerve-cells wherever they exist. Its intimate nature is, therefore, a problem co-extensive with the existence of nerve-cells, and enters as a factor into every question concerning the specific reactions of the nervous system.

Thirdly, physiology seeks in the nervous system how by its 'conductivity' the separate units of an animal body are welded into a single whole, and from a mere collection of organs there is constructed an individual animal.

This third line of inquiry, though greatly needing more data from the second and the first, must in the meantime go forward of itself. It is at present busied with many questions that seem special—hence its work is generally catalogued as special physiology. But it includes general problems. In the time before us I would venture to put before you one of these.

When we regard the nervous system as to this, which I would term its *integrative* function, we can distinguish two main types of system according to the mode of union of the conductors—(i.) the *nerve-net* system, such as met in medusa and in the walls of viscera, and (ii.) the *synaptic* system, such as the cerebro-spinal system of arthropods and vertebrates. In the integrative function of the nervous system the unit mechanism is the *reflex*. The chain of conduction in the reflex

is a nervous arc, running from a receptor organ to an effector organ, *e. g.*, from a sense-organ to a limb-muscle. We may still, I think, conveniently accept the morphological units termed neurones as units of construction of the reflex arc. It may be that these neurones are in some cases not unicellular, but pluricellular. That question need not detain us now. Accepting the neurone as the unit of structure of the reflex chain, the characteristic of the synaptic system is that the chain consists of neurones jointed together in such a way that conduction along the chain seems possible in one direction only. These junctions of the neurones are conveniently termed synapses. The irreversible direction of the conductivity along the neurone chain is probably referable to its synapses. This irreciprocity of conduction especially distinguishes the synaptic nervous system from the nerve-net system. That neurone forms the sole avenue which impulses generated at its receptive point can use whithersoever may be their distant destination. That neurone is therefore a path exclusive to the impulses generated at its own receptive points, and other receptive points than its own can not employ it.

But at the termination of every reflex arc we find a final neurone, the ultimate conductive link to an effector organ, gland or muscle. This last link in the chain, *e. g.*, the motor neurone, differs obviously in one important respect from the first link of the chain. It does not subserve exclusively impulses generated at one single receptive source alone, but receives impulses from many receptive sources situate in many and various regions of the body. It is the sole path which all impulses, no matter whence they come, must travel if they would reach the muscle fibers which it joins. Therefore, while the receptive neurone forms a private path exclusive for impulses of one source only, the final or efferent neurone is, so to say, a public path, *common* to impulses arising at any of many sources in a variety of receptive regions of the body. The same effector organ stands in reflex connection not only with many individual receptive points, but even with many various receptive *fields*. Reflex arcs arising in manifold sense organs can pour their influence into one and the same muscle. A limb muscle is the *terminus ad quem* of nervous arcs arising not only in the right eye, but in the left; not only in the eyes, but in the organs of smell and hearing; not only in these, but in the geotropic labyrinth, in the skin and in the muscles and joints of the limb itself and of the other limbs as well. Its motor nerve is a path common to all these.

Reflex arcs show, therefore, the general feature that the initial neurone is a private path exclusive for a single receptive point; and that finally the arcs embouch into a path leading to an effector organ, and that this final path is common to all receptive points wheresoever they may lie in the body, so long as they have any connection at all with

the effector organ in question. Before finally converging upon the motor neurone arcs usually converge to some degree by their private paths embouching upon internuncial paths common in various degree to groups of private paths. The terminal path may, to distinguish it from internuncial common paths, be called *the final common path.* The motor nerve to a muscle is a collection of such final common paths.

Certain results flow from this arrangement. One seems the preclusion of qualitative differences between nerve-impulses arising in different afferent nerves. If two conductors have a tract in common, there can hardly be qualitative difference between their modes of conduction.

A second result is that each receptor being independent for communication with its effector organ upon a path not exclusively its own, but common to it with certain other receptors, that nexus necessitates successive and not simultaneous use of the common path by various receptors using it to different effect.

The first link of each reflex chain is a neurone which starts in a receptor organ, *e. g.,* a sense-organ. A receptive field, *e. g.,* an area of skin, is always analyzable into receptive points, and the initial nerve-path in every reflex arc starts from a receptive point or points. A single receptive point may play reflexly upon quite a number of different effector organs. It may be connected through its reflex path with many muscles and glands in various parts. Yet all its reflex arcs spring from the one single shank, so to say; that is, from the one afferent neurone that conducts from the receptive point at the periphery into the central nervous organ. This neurone dips at its deep end into the great central nervous organ, the cord or brain. There it enters a vast network of conductive paths. In this network it forms manifold connections. So numerous are its potential connections there, that, as shown by the general convulsions induced under strychnia-poisoning, its impulses can discharge practically every muscle and effector organ in the body. Yet under normal circumstances the impulses conducted by it to this central network do not irradiate there in all directions. Though their spread over the conducting network does, as judged by the effects, increase with increase of stimulation of the entrant path, the irradiation remains limited to certain lines. Under weak stimulation of the entrant path these lines are sparse. The conductive network affords, therefore, to any given path entering it some communications that are easier than others. This canalization of the network in certain directions from each entrant point is sometimes expressed, borrowing electrical terminology, by saying that the conductive network from any given point offers less resistance along certain circuits than along others. This recognizes the fact that the conducting paths in the great central organ are arranged in a particu-

lar pattern. The pattern of arrangement of the conductive network of the central organ reveals somewhat of the integrative function of the nervous system. It tells us what organs work together in time. The impulses are led to this and that effector organ, gland or muscle, in accordance with the pattern. The success achieved in the unraveling of the conductive patterns of the brain and cord is shown by the diagrams furnished by the works of such investigators as Edinger, Exner, Flechsig, van Gehuchten, v. Lenhossek, v. Monakow, Ramon and Schäfer. Knowledge of this kind stands high among the neurological advances of our time.

But we must not be blind to its limitations. The achievement may, though more difficult, be likened to tracing the distribution of blood vessels after Harvey's discovery gave them meaning, but before the vasomotor mechanism was discovered. The blood vessels of an organ may be turgid at one time, constricted almost to obliteration at another. With the conductive network of the nervous system the temporal changes are even greater, for they extend to absolute withdrawal of nervous influence. Our schemata of the pattern of the great central organ take no account of temporal data. But the pattern of the web of conductors is not really immutable. Functionally its details change from moment to moment. In any active part it is a web that shifts from one pattern to another, from a first to a second, from a second to a third, then back perhaps to the first and then to a fourth and so on backwards and forwards. As a tap to a kaleidoscope, so a new stimulus that strikes the central organ causes it to assume a partially new pattern. The pattern in general remains, but locally the patterns are in constant flux of back and forward change. These time-changes offer, I venture to think, a study important for understanding the integrative function of the nervous system.

If we regard the nervous system of any higher organism from the broad point of view, a salient feature in its architecture is the following: At the commencement of every reflex arc is a receptive neurone, extending from the receptive surface to the central nervous organ.

INVENTION AND DISCOVERY.

By Hon. CHARLES A. PARSONS, M.A., F.R.S., M.Inst.C.E.,
PRESIDENT OF THE ENGINEERING SECTION OF THE BRITISH ASSOCIATION.

ON this occasion I propose to devote my remarks to the subject of invention.

It is a subject of considerable importance, not only to engineers, but also to men of science and the public generally.

I also propose to treat invention in its wider sense, and to include under the word discoveries in physics, mechanics, chemistry and geology.

Invention throughout the middle ages was held in little esteem. In most dictionaries it receives scant reference except as applied to poetry, painting and sculpture.

Shakespeare and Dryden describe invention as a kind of muse or inspiration in relation to the arts, and when taken in its general sense to be associated with deceit, as 'Return with an invention, and clap upon you two or three plausible lies.'

As to the opposition and hostility to scientific research, discovery and mechanical invention in the past, and until comparatively recent times, there can be no question, in some cases the opposition actually amounting to persecution and cruelty.

The change in public opinion has been gradual. The great inventions of the last century in science and the arts have resulted in a large increase of knowledge and the powers of man to harness the forces of nature. These great inventions have proved without question that the inventors in the past have, in the widest sense, been among the greatest benefactors of the human race. Yet the lot of the inventor until recent years has been exceptionally trying, and even in our time I scarcely think that any one would venture to describe it as altogether a happy one. The hostility and opposition which the inventor suffered in the middle ages have certainly been removed, but he still labors under serious disability in many respects under law as compared with other sections of the community. The change of public feeling in favor of discovery and invention has progressed with rapidity during the last century. Not only have private individuals devoted more time and money to the work, but societies, institutions, colleges, municipalities and governments have founded research laboratories, and in some instances have provided large endowments. These measures have increased the number of persons trained to scientific methods, and also

provided greatly improved facilities for research; but perhaps one of the most important results to engineers has been the direct and indirect influence of the more general application of scientific methods to engineering.

Sir Frederick Bramwell, in his presidential address to this association in 1888, emphasized the interdependence of the scientist and the civil engineer, and described how the work of the latter has been largely based on the discoveries of the former; while the work of the engineer often provides data and adds a stimulus to the researches of the scientist. And I think his remarks might be further appropriately extended by adding that since the scientist, the engineer, the chemist, the metallurgist, the geologist, all seek to unravel and to compass the secrets of nature, they are all to a great extent interdependent on each other.

But though research laboratories are the chief centers of scientific invention, and colleges, institutions and schools train the mind to scientific methods of attack, yet in mechanical, civil and electrical engineering the chief work of practical investigation has been carried on by individual engineers, or by firms, syndicates and companies. These not only have adapted discoveries made by scientists to commercial uses, but also in many instances have themselves made such discoveries or inventions.

To return to the subject, let us for a moment consider in what invention really consists, and let us dismiss from our minds the very common conception which is given in dictionaries and encyclopedias that invention is a happy thought occurring to an inventive mind. Such a conception would give us an entirely erroneous idea of the formation of the great steps in advance in science and engineering that have been made during the last century; and, further, it would lead us to forget the fact that almost all important inventions have been the result of long training and laborious research and long-continued labor. Generally, what is usually called an invention is the work of many individuals, each one adding something to the work of his predecessors, each one suggesting something to overcome some difficulty, trying many things, testing them when possible, rejecting the failures, retaining the best, and by a process of gradual selection arriving at the most perfect method of accomplishing the end in view.

This is the usual process by which inventions are made.

Then after the invention, which we will suppose is the successful attempt to unravel some secret of nature, or some mechanical or other problem, there follows in many cases the perfecting of the invention for general use, the realization of the advance or its introduction commercially; this after-work often involves as great difficulties and requires for its accomplishment as great a measure of skill as the inven-

tion itself, of which it may be considered in many cases as forming a part.

If the invention, as is often the case, competes with or is intended to supersede some older method, then there is a struggle for existence between the two. This state of things has been well described by Mr. Fletcher Moulton. The new invention, like a young sapling in a dense forest, struggles to grow up to maturity, but the dense shade of the older and higher trees robs it of the necessary light. If it could only once grow as tall as the rest all would be easy, it would then get its fair share of light and sunshine. Thus it often occurs in the history of inventions that the surroundings are not favorable when the first attack is made, and that subsequently it is repeated by different persons, and finally under different circumstances it may eventually succeed and become established.

We may take in illustration almost any of the great inventions of undoubted utility of which we happen to have the full history—for instance, of some of the great scientific discoveries, or some of the great mechanical inventions, such as the steam-engine, the gas-engine, the steamship, the locomotive, the motor-car or some of the great chemical or metallurgical discoveries. Are not most, if not all, of these the result of the long-continued labor of many persons, and has not the financial side been, in most cases, a very important factor in securing success?

The history of the steam-engine might be selected, but I prefer on this occasion to take the internal-combustion engine, for two reasons—firstly, because its history is a typical one; and secondly, because we are to hear a paper by that able exponent and great inventor in the domain of the gas-engine, Mr. Dugald Clark, describing not only the history, but the engine in its present state of development and perfection, an engine which is able to convert the greatest percentage of heat units in the fuel into mechanical work, excepting only, as far as we at present know, the voltaic battery and living organisms.

The first true internal-combustion engine was undoubtedly the cannon, and the use in it of combustible powder for giving energy to the shot is strictly analogous to the use of the explosive mixture of gas or oil and air as at present in use in all internal-combustion engines; thus the first internal-combustion engine depended on the combination of a chemical discovery and a mechanical invention, the invention of gunpowder and the invention of the cannon.

In 1680 Huygens proposed to use gunpowder for obtaining motive power in an engine. Papin, in 1690, continued Huygens's experiments, but without success. These two inventors, instead of following the method of burning the powder under pressure, as in the cannon, adopted, in ignorance of the thermodynamic laws, an erroneous course.

They exploded a small quantity of gunpowder in a large vessel with escape valves, which after the explosion caused a partial vacuum to remain in the vessel. This partial vacuum was then used to actuate a piston or engine and perform useful work. Subsequently several other inventors worked on the same lines, but all of these failed on account of two causes which now are very evident to us. Firstly, gunpowder was then, as it still is, a very expensive form of fuel, in proportion to the energy liberated on explosion; secondly, the method of burning the powder to cause a vacuum involves the waste of nearly the whole of the available energy, whereas, had it been burned under pressure, as in the cannon, a comparatively large percentage of the energy would have been converted into useful work. But even with this alteration, and however perfect the engine had been, the cost of explosives would have debarred its coming into use, except for very special purposes.

We come a century later to the first real gas-engine. Street, in 1794, proposed the use of vapor of turpentine in an engine on methods closely analogous to those successfully adopted in the Lenoir gas-engine of eighty years later, or thirty years ago. But Street's engine failed from crude and faulty construction. Brown, in 1823, tried Huygens's vacuum method, using fuel to expand air instead of gunpowder, but he also failed, probably on account of the wastefulness of the method.

Wright, in 1833, made a really good gas-engine, having many of the essential features of some of the gas-engines of the present day, such as separate gas and water pumps, and water-jacketed cylinder and piston.

Barnett, in 1839, further improved on Wright's design, and made the greatest advance of any worker in gas-engines. He added the fundamental improvements of compression of the explosive mixture before combustion, and he devised means of lighting the mixture under pressure, and his engine conformed closely to the present-day practise as regards fundamental details. No doubt Barnett's engine, so perfect in principle, deserved commercial success, but either his mechanical skill or his financial resources were inadequate to the task, and the character of the patents would seem to favor this conclusion, both as regards Barnett and other workers at this period. Up to 1850 the workers were few, but as time went on they gradually increased in number; attention had been attracted to the subject, and men with greater powers and resources appear to have taken the problem in hand. Among these numerous workers came Lenoir, in 1860, who, adopting the inferior type of non-compression engine, made it a commercial success by his superior mechanical skill and resources. Mr. Dugald Clerk tells us: "The proposals of Brown (1823), Wright (1833), Barnett (1838), Bansanti and Matteucci (1857), show gradually increasing knowledge of detail and the difficulties to be overcome, all leading to the first practicable engine in 1866, the Lenoir." This stage of the

development being reached, the names of Siemens, Beaude, Roches, Otto Simon, Dugald Clerk, Priestman, Daimler, Dowson, Mond and others appear as inventors who have worked at and added something to perfect the internal-combustion engine and its fuel, and who have helped to bring it to its present state of perfection.

In the history of great mechanical inventions there is perhaps no better example of the interdependence of the engineer, the physicist and the chemist than is evidenced in the perfecting of the gas-engine. The physicist and the chemist together determine the behavior of the gaseous fuel, basing their theory on data obtained from the experimental engines constructed by the mechanical engineer, who, guided by their theories, makes his designs and improvements; then, again, from the results of the improvements fresh data are collected and the theory further advanced, and so on till success is reached. But though I have spoken of the physicist, the chemist and the engineer as separate persons, it more generally occurs that they are rolled into one, or at most two, individuals, and that it is indispensable that each worker should have some considerable knowledge of all the sciences involved to be able to act his part successfully.

Now let us ask: Could not this very valuable invention, the internal-combustion engine, have been introduced in a much shorter time by more favoring circumstances, by some more favorable arrangement of the patent laws, or by legislation to assist the worker attacking so difficult a problem? I think the answer is that a great deal might be done, and I will endeavor to indicate some changes and possible improvements.

The history of this invention brings before our minds two important considerations. Firstly, let us consider the patentable matter involved in the invention of the gas-engine, the utilization for motive-power purposes of the then well-known properties of the explosive energy of gunpowder or of mixture of gas and oil with air. Are not these obvious inferences to persons of a mechanical turn of mind and who had seen guns fired, or explosions in bottles containing spirits of turpentine when slightly heated and a light applied to the neck? Surely no fundamental patent could have been granted under the existing patent laws for so obvious an application of known forces. Consequently, patent protection was sought in comparative details, details in some cases essential to success which were evolved or invented in the process of working out the invention. In this extended field of operations a slight protection was in some instances obtained. But in answer to the question whether such protection was commensurate with the benefits received by the community at large, there can, I think, be only one reply. Generally, those who did most got nothing, some few received insufficient returns, and in very few cases indeed can the return

be said to have been adequate. The second important consideration is that of the methods of procedure of the patentees, for it appears that very few of them had studied what had been suggested or done before by others before taking out their own patent. We are also struck by the number of really important advances that have been suggested and have failed to fructify, either from want of funds or other causes, to be forgotten for the time and to be re-invented later on by subsequent workers.

What a waste of time, expense and disappointment would be avoided if we in England helped the patentee to find out easily what had been done previously, on the lines adopted by the United States and German Patent Offices, who advise the patentee after the receipt of his provisional specification of the chief anticipatory patents, dead or alive! And ought we in England to rest content to see our patentees awaiting the report of the United States and German Patent Offices on their foreign equivalent specifications before filing their English patent claims? Ought not our Patent Office to give more facilities and assistance to the patentee?

Before proceeding further to discuss some of the possible improvements for the encouragement and protection of research and invention, I ask you to further consider the position of the inventor—the man anxious to achieve success where others have hitherto failed. To be successful he must be something of an enthusiast; and usually he is a poor man, or a man of moderate means, and dependent on others for financial assistance. Generally the problem to be attacked involves a considerable expenditure of money; some problems require great expenditure before any return can thereby accrue, even under the most favorable circumstances. In the very few cases where the inventor has some means of his own they are generally insufficient to carry him through, and there have unfortunately been many who have lost everything in the attempt. In nearly all cases the inventor has to cooperate with capital: the capitalist may be a sleeping partner, or the capital may be held by a firm or syndicate, the inventor in such cases being a partner—a junior partner—or a member of the staff. The combination may be successful and lasting, but unfortunately the best inventors are often bad men of business. The elements of the combination are often unstable, and the disturbing forces are many and active; especially is this so when the problem to be attacked is one of difficulty, necessitating various and successive schemes involving considerable expenditure, generally many times greater than that foreshadowed at the commencement of the undertaking. Under such circumstances, unless the capitalist or senior partner or board be in entire sympathy with the inventor or exercise great forbearance, stimulated by the hope of ultimate success and adequate returns, the case

becomes hopeless, disruption takes place, and the situation is abandoned. Further, in the majority of cases, after some substantial progress has been made it is found that under the existing patent laws insufficient protection can be secured, and the prospect of a reasonable return for the expenditure becomes doubtful. Under such circumstances the capitalist will generally refuse to proceed further unless the prospect of being first in the field may tempt him to continue.

Very many inventors, as I have said, avoid the expense of searching the patent records to see how far their problem has been attacked by others. In some cases the cost of a thorough search is very great indeed; sometimes it is greater than the cost of a trial attack on the problem. In the case of young and inexperienced inventors there sometimes exists a disinclination to enter on an expensive search; they prefer to spend their money on the attack itself. There are some, it is true, who have a foolish aversion to take steps to ascertain if others have been before them, and who prefer to remain in ignorance and trust to chance. It will, however, be said that the United States and German Patent Office reports ought to suffice to warn or protect the English patentee; but my own experience has been that such protection is not entirely satisfactory. There is, firstly, a considerable interval before such reports are received, and the life of a patent is short. Then, if the patent is upon an important subject, attracting general attention, the search is vigorous and sometimes overwrought, and the patent unjustly damaged or refused altogether. If, however, the patent is on some subject not attracting general attention, it receives too little attention and is granted without comment.

THE PROGRESS OF SCIENCE.

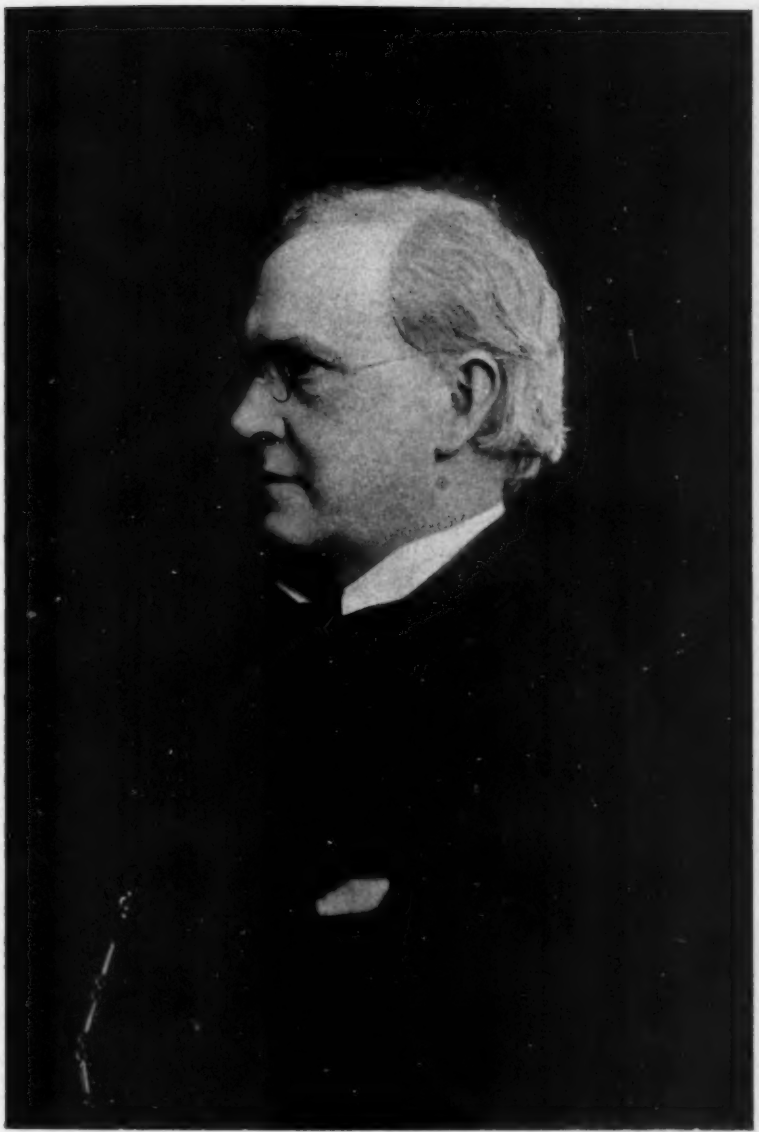
THE CAMBRIDGE MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE meeting of the British Association at Cambridge was an event of sufficient scientific importance to deserve attention here as well as in Great Britain. We are pleased, therefore, to be able to devote the present number of the MONTHLY to it. President Pritchett, of the Massachusetts Institute of Technology, contributes an interesting account of the general features of the meeting. The address of the president of the association and of the president of the section of mathematics and physics are printed in full, and parts of other addresses are given. These addresses appear to give the best available survey of the range and problems of modern science. It is possible that some of them are in part too technical for the purposes of the general scientific reader, but he must meet the man of science half way. The difficulty is more in the terminology than in the ideas. When the pages are scattered over with statoliths and gametes there is a natural tendency to skip rather than to add to our usable vocabulary. But the entire terminology needed to understand the main results of modern science is not more difficult than one foreign language and not more extensive than that of the sporting field. The language of science should be acquired by people of intelligence, but at the same time men of science should learn on occasion to address those who are not specialists.

In addition to the presidential addresses before the sections, the British Association makes arrangements for several still more popular lectures, one addressed explicitly to 'working-men.' This lecture was given by Dr. J. E. Marr, of Cambridge, his subject being the 'Forms of Mountains.' Other lectures were given on 'Ripple Marks and Sand Dunes,' by Professor George Darwin; on 'The Origin and Growth of Cambridge University,' by Mr. J. W. Clark, and on 'Recent Paleontological Exploration,' by Professor H. F. Osborn, of Columbia University. On the other hand, the papers before the sections were mostly technical in character, though geography, anthropology, political science and education always give occasion for popular papers and discussions.

The entertainments and excursions at meetings of the British Association are always well arranged; it has indeed been urged that they are too attractive to camp followers. The social conditions in Great Britain are favorable to dinners and garden parties, and there is nearly always a duke or at least a lord ready to offer hospitalities. At Cambridge the entertainments were naturally academic in character, the principal functions being at Trinity and St. John's Colleges. Honorary degrees were conferred on some fifteen of the visitors, America being represented by Professor Osborn.

Cambridge proved to be an attractive place for the meeting, and no wonder, for a large part of the active British scientific workers have studied there, and the university unites scientific preeminence and medieval charm. The registration was 2,783, the sixth largest in the history of the association, and representing probably the largest gathering of scientific men. At Manchester and Liverpool, where the largest meetings have been held, the registration is swollen by local associates who pay the fees without taking



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HORACE LAMB, LL.D., F.R.S.

Professor of Pure Mathematics, Owen's College, Manchester. President of the Mathematical and Physical Section.

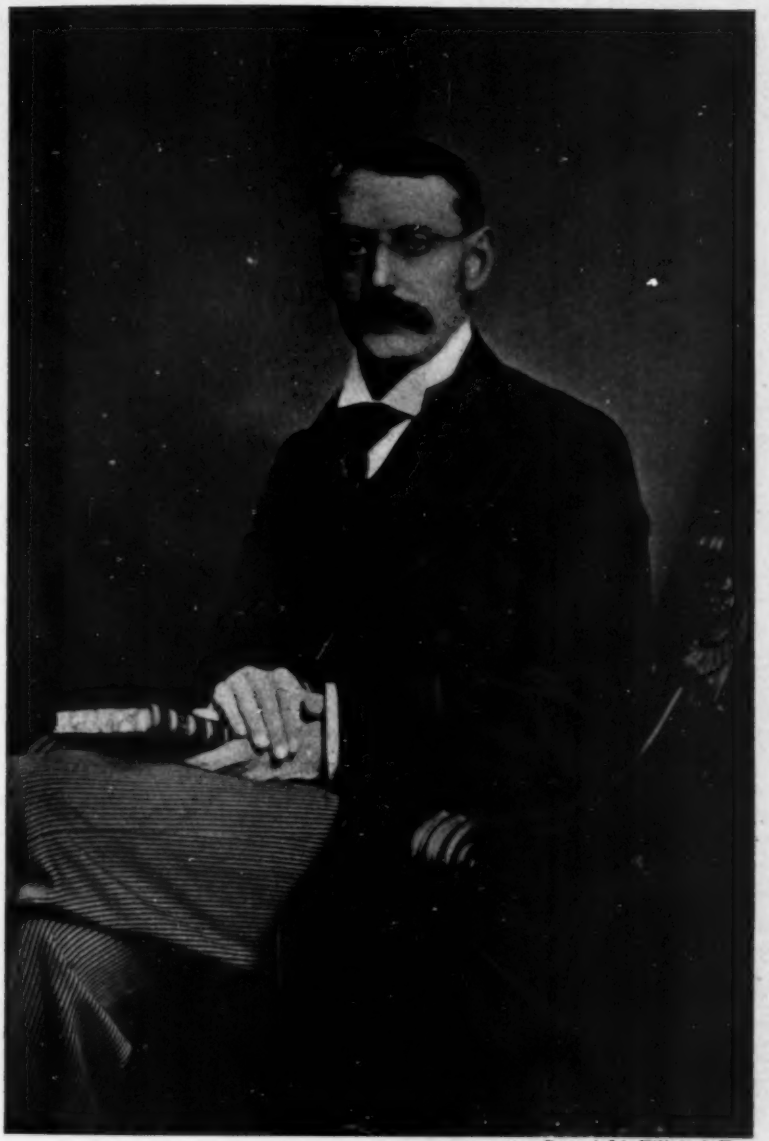
much if any part in the sessions. The meeting next year will be in South Africa under the presidency of Professor George H. Darwin, who holds the chair of astronomy at Cambridge.

THE ADDRESS OF THE PRESIDENT.

It is as long as forty-two years since the association last met at Cambridge. There was a meeting at Oxford ten years ago after an interval of thirty-four years. The association was organized at York, in 1831, and held its second meeting at Oxford and its third meeting at Cambridge. It met again at Oxford in 1847 and 1860 and at Cambridge in 1845 and 1862. Some of the more conservative elements at the great universities have apparently not altogether welcomed the influx of visitors brought together by the diverse attractions of a meeting of the association. But if the meetings at the seats of the universities have not been frequent, they have tended to be of rather more than usual interest. The very contrast between the conservative and somewhat aristocratic attitude of the universities and the more radical and democratic aspects of the association have given an element of dramatic interest. Thus the Oxford meeting of 1860 is remembered for the invasion of the Darwinian theory. Bishop Wilberforce had been attacking the then infant theory in a superficial and rhetorical manner and is alleged to have turned to Huxley and asked him whether it was through his grandfather or his grandmother that he claimed his descent from a monkey. Huxley, after stating the case for evolution, said that a man has no reason to be ashamed of having descended from an ape, but that he would be ashamed to be related to a man who used his ability and position to obscure the truth. No wonder that it was thirty-four years before the association was again invited to meet at the home of 'lost causes and impossible loyalties,' nor is it surprising that it should then have been presided over by a great statesman, who once more attempted to discredit evolution and the Darwinian theory.

Now after ten years the association meets at the sister university, and is presided over by the nephew and successor as head of the British government of the president of the Oxford meeting. It is a strange sight as viewed from this land of magnificent levels. Mr. Balfour has character and abilities as notable and an intellect even more acute than had Lord Salisbury; there is a curious similarity in the attitude of the two men toward science, but at the same time an obvious forward movement in the authority of science when we pass from Oxford to Cambridge, from the older to the younger generation.

Lord Salisbury told his audience bluntly that he would make a survey 'not of our science but of our ignorance' and instanced atoms, the ether and the origin of life. He rejected natural selection and ended by appealing to the intelligent and benevolent design of an everlasting creator and ruler. Mr. Balfour is too acute a thinker to set natural selection and design in opposition to one another. He, indeed, takes natural selection for granted and uses it to discredit the possibility of knowledge. He tells us that natural selection working through utility could only give us the senses needed 'to fight, to eat and to bring up children,' not the intellect required to understand physical reality. The more successful we are in explaining the origin of our beliefs 'the more doubt we cast on their validity.' Mr. Balfour, however, is ready to sanction the newest theories of physical metaphysics, saying that down to five years ago 'our race has, without exception, lived and died in a world of illusions'; now apparently Mr. Balfour and a handful of physicists live in a real world of ether and electrical monads. But perhaps Mr. Balfour is a bit ironical, and is



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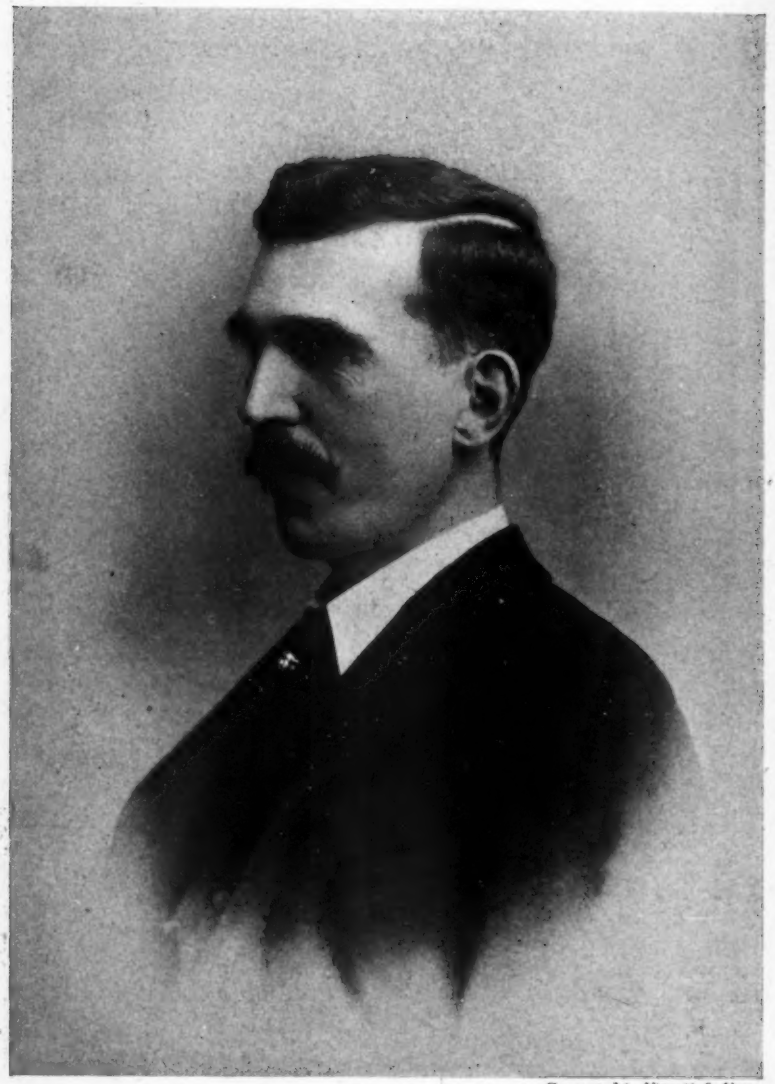
THE HON. CHARLES PARSONS, F.R.S.
President of Engineering Section.



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SYDNEY YOUNG, F.R.S.

Professor of Chemistry, Trinity College, Dublin. President of the Chemical Section.

only poking fun at simple scientific folks. The outcome of the two addresses, clearly indicated in the one subtly implied in the other, is that our science having failed, we might as well accept the doctrines of the established church.

Mr. Balfour's address will be read with interest by all. The more difficult but very able address of Professor Lamb, also published in this number of THE POPULAR SCIENCE MONTHLY, should, however, be read in connection with it. As Professor Lamb tells us, science is justified by its results; we trust it because it honors our checks.

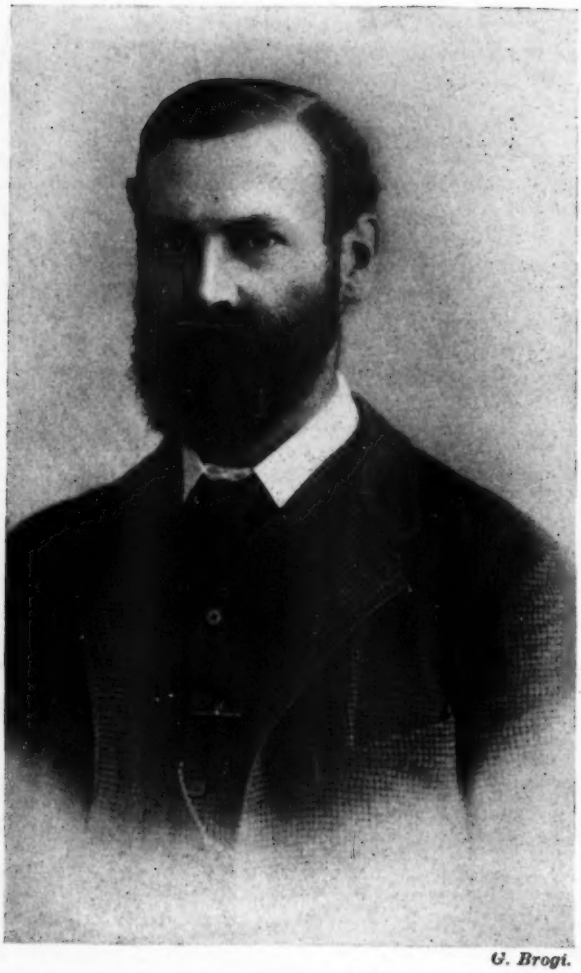
THE WORK OF THE SECTIONS.

THE British Association is divided into ten sections, which are at times subdivided. There were for example this year subsections for cosmical physics and for agriculture. The range of the British Association is somewhat wider than that of the American Association. Our association has recently established a section for experimental medicine, but this has not been active, whereas physiology at the British Association has one of the best sectional programs. We have no section for education and our section for political and social science is not very strong. These two sections are likely to offer papers and discussions that are only on the edge of science, but with the sections of geography and anthropology they give suitable entertainment to the 'general hearer,' whose cooperation in scientific work it should be one of the objects of such an association to secure.

The most interesting part of the program of Section A, and perhaps the most important discussion of the meeting from the scientific point of view, was that on the radio-activity of ordinary matter, in which Professor J. J. Thomson, Lord Kelvin, Lord Rayleigh and Sir Oliver Lodge took part, four physicists whose work can scarcely be paralleled in any other country. Professor Thomson, to whom the new theories in regard to matter are so largely due, described work that had been carried on in his laboratory. It has been found that metals give out radiations peculiar to each metal. This can scarcely be due to the presence of a small quantity of radium as it is constant with different samples of the same metal. Sir Oliver Lodge remarked that on the electric theory of matter all matter ought to be radio-active, and no atom of matter should be regarded as absolutely permanent. Perhaps it would not be going too far to say that the burden of proof rested with those who denied that ordinary matter was radio-active.

Physics is undoubtedly stronger in Great Britain than in the United States, and the Cavendish Laboratory at Cambridge has through the discoveries of Maxwell, Rayleigh and Thomson become the chief center for physical investigation in the world. It is natural, therefore, that this science should have been well represented at the recent meeting. Dr. Glazebrook, director of the National Physical Laboratory, described its work. He told of the part taken by the British Association in its establishment, described the scientific and technical work in progress and concluded by pleading for a larger measure of support. A table was submitted showing the amount of expenditure on buildings and equipment, and of the annual grant allowed for maintenance in similar institutions in various countries; this clearly demonstrated the unfortunate position in which the laboratory was placed, while the number of tests made and the receipts from applicants contrasted favorably with those of other nationalities.

Among other contributions of special interest was a paper by Professor J. A. Fleming on the propagation of electric waves along spiral wires and on an appliance for measuring the length of waves used in wireless telegraphy,



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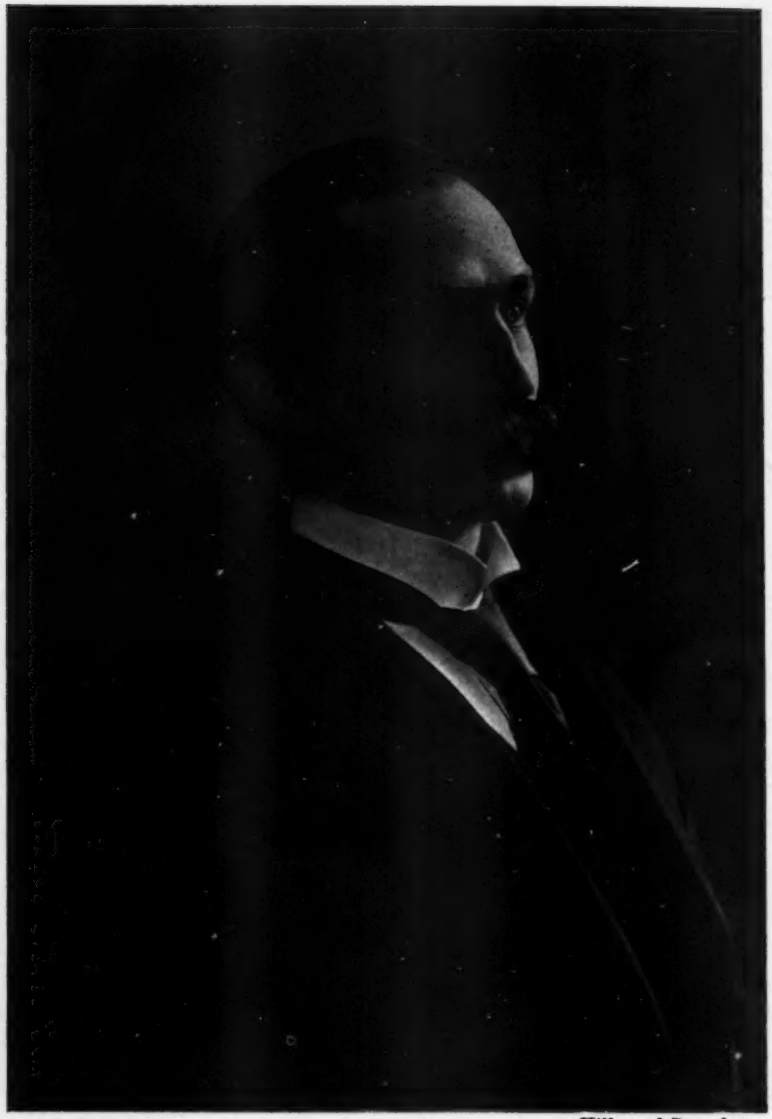
AUBREY STRAHAN, F.R.S.

District Geologist, Geological Survey of the United Kingdom. President of the Geological Section.

and an address by Professor J. H. Poynting on 'Radiation in the solar system.' A number of papers were presented to the section by foreign visitors including two by Professor Wood, of the Johns Hopkins University, one describing some recent improvements in the diffraction process of color photography and another on the anomalous dispersion of sodium vapor. Section A includes mathematics and astronomy as well as physics. Among the papers was one by Sir David Gill, director of the observatory at the Cape of Good Hope, entitled 'Problems of Astronomy,' which discussed some of the questions in practical astronomy now pressing for solution.

Before the Chemical Section Sir James Dewar gave an address on very low temperature phenomena with interesting demonstrations. He showed by a series of beautiful vacuum-tube experiments the behavior of nitrogen, oxygen, hydrogen and argon when submitted in contact with charcoal to the cooling influence of liquid air. The nitrogen tube exhibited the usual violet color when the spark passed through it, but as soon as the charcoal was immersed in the liquid air the tube passed through various stages of attenuated brilliancy until ultimately the vacuum became so high that the current could scarcely overcome the resistance. When the liquid air was removed the changes were passed through in the reverse order. Oxygen passed through a similar series of changes, but in the case of hydrogen the absorptive power of the charcoal at the temperature of liquid air was not great enough to render the tube non-conductive. Lord Kelvin, in proposing a vote of thanks to Professor Dewar, said that he was filled with expectation as to the condition of things that would be disclosed at a temperature of 5° absolute, where there would be no motion. What would become of electric conductivity, of magnetism, of thermal conductivity? He wished Sir James would continue his electro-conductivity experiments and bring him copper highly conductive at 8° but as great an insulator as glass at one or two or three degrees. Sir William Ramsay, speaking on the changes produced by the β-rays, said that he had obtained 105 milligrams of radium bromide, which, being too precious to risk in one vessel, were divided amongst three bulbs. These bulbs were placed in glass vessels, and were each provided with a tube to take away emanations. The vessels were colorless to start with, and were some of potash and some of soda glass, but in course of time the former became brown and the latter violet in color. The glass, too, became radio-active, but this property was removed by washing with water, although the color remained. When a solution of radium bromide was evaporated an invisible residue was left which was radio-active and dissolved to a radio-active solution. Radium formed chloride, sulphide, hydroxide and sulphate similar to lead, except that they were radio-active. The radium emanations rendered silver and platinum as well as glass radio-active. Professor W. O. Atwater, of Wesleyan University, presented two papers, one on the agricultural experimental work in the Smithsonian Institution and one on the experiments in nutrition carried out under his direction.

Mr. Aubrey Strahan's presidential address before the geological section was on earth movements, and this was taken as a subject for special discussion. In opening it Mr. Straham said that the subject proposed for discussion was the nature and origin of those movements of the earth's crust which have manifested themselves in the factoring, overthrusting and folding of strata. These movements have been in operation from the earliest to the latest geological periods; and, though they have been intermittent so far as any one region is concerned, there is reason to believe that they have been more or



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HENRY BALFOUR, M.A.
President of the Anthropological Section.

less continuously in action throughout the world as a whole. Their operations, in fact, are essential to the existence of a land surface, for in their absence all rocks projecting above the sea would be worn away, and the globe would become enveloped in one continuous ocean. Notwithstanding these facts, and though they have been the object of prolonged study, no theory as to the cause of the movements has commanded universal acceptance. While some hold that the shrinking of the globe by cooling and the efforts of the crust to adapt itself to the shrinking interior are the prime causes, others maintain that the scale on which folding and overthrusting in the crust have taken place is out of all proportion to the shrinking that can be attributed to such a cause. During the meeting the collections of fossils and rocks in the new Sedgwick Museum, adjoining the hall in which the section's proceedings took place, were open and largely visited by members.

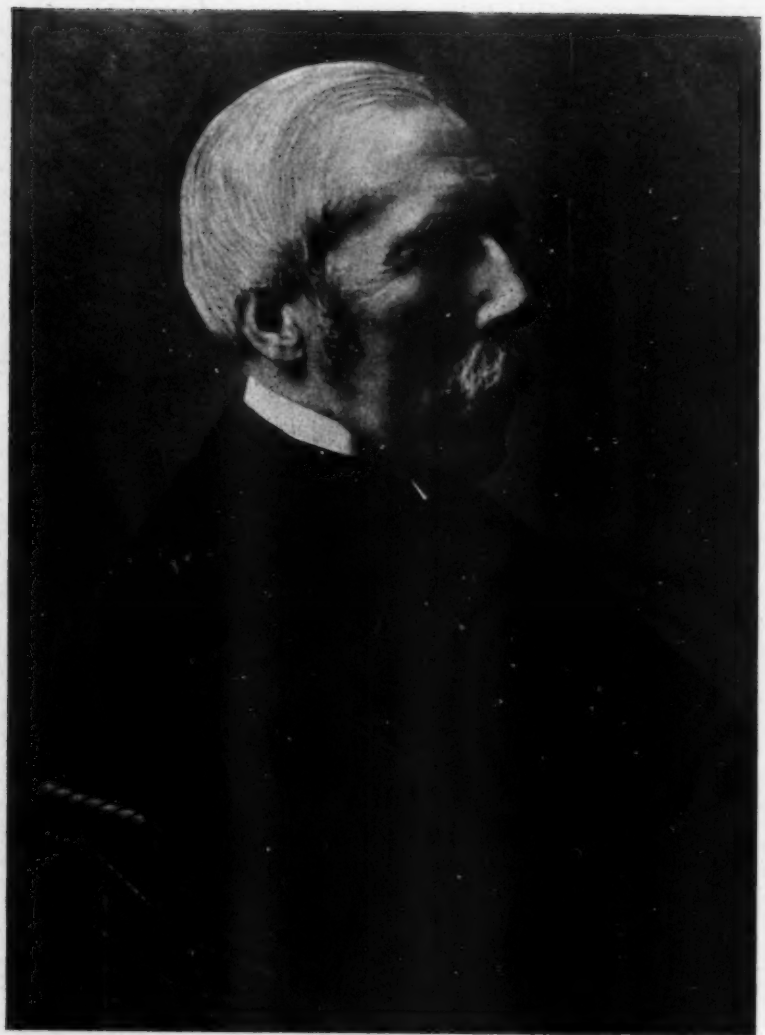
Questions of heredity and experimental breeding were prominent in the proceedings of Section D as well as in the president's address. Exhibits were included, thus Miss Saunders showed a selection of stocks; Mr. Bateson, fowls and sweet peas; Mr. Darbishire, mice; Mr. Hurst, rabbits; Mr. Staples-Browne, pigeons; Mr. Doncaster, the Aleraxas moth; Mr. Locke, maize; and Mr. Biffers, wheat. Professor H. F. Osborn gave a lantern lecture on the evolution of the horse, and Professor J. C. Ewart and Professor W. Ridgeway described their investigations on the same subject. Professor Ewart's Celtic ponies, to which allusion was made, were on view in the court adjoining the Sedgwick Museum of Geology. Professor E. B. Poulton, of Oxford, delivered an address on the mimetic resemblance of Diptera for Hymenoptera, and Professor Gary N. Calkins, of Columbia University, one on the germ of smallpox. Other subjects having medical as well as strictly zoological interest were accounts of miner's worm and cancer research. Professor C. S. Minot, of Harvard University, read papers on regeneration, telegony and the Harvard embryological collection.

Before the Geographical Section there were several popular illustrated lectures, one on the work of the Scottish Antarctic Expedition, by Mr. W. S. Bruce, its leader; one by Mr. Silva White, on the unity of the Nile Valley, and one by Dr. Tempest Anderson, on the Lipari Islands and their Volcanoes.

The next section in the alphabetical order, Economic and Social Science, also contained a good many popular addresses and papers. The topic for special discussion was the housing of the poor by municipalities, but free trade and protection were naturally prominent in view of the present national interests.

The sections for engineering and physiology were the most technical in the character of their discussions. In the latter Sir John Burdon-Sanderson opened an important discussion on oxidation and functional activity, or the relation between oxygen and the chemical processes of animal and plant life. Experimental psychology was included under physiology. In botany Professor H. Marshall Ward and Professor Jakob Eriksson, of Stockholm discussed recent work on the biology of the fungi, especially the uredineae, and Dr. F. F. Blackman gave an account of his experimental researches on the assimilation and respiration of plants. In a paper of general interest Lord Avebury discussed the forms of stems of plants, showing that they anticipated engineering work in the economical use of the strength of materials.

The establishment of a Section of Education has added considerably to the popular interest of the meetings. Among the subjects under discussion were school leaving certificates; the national and local provisions for the training of teachers, and manual instruction in schools. Discussions took place on the reports of committees; one



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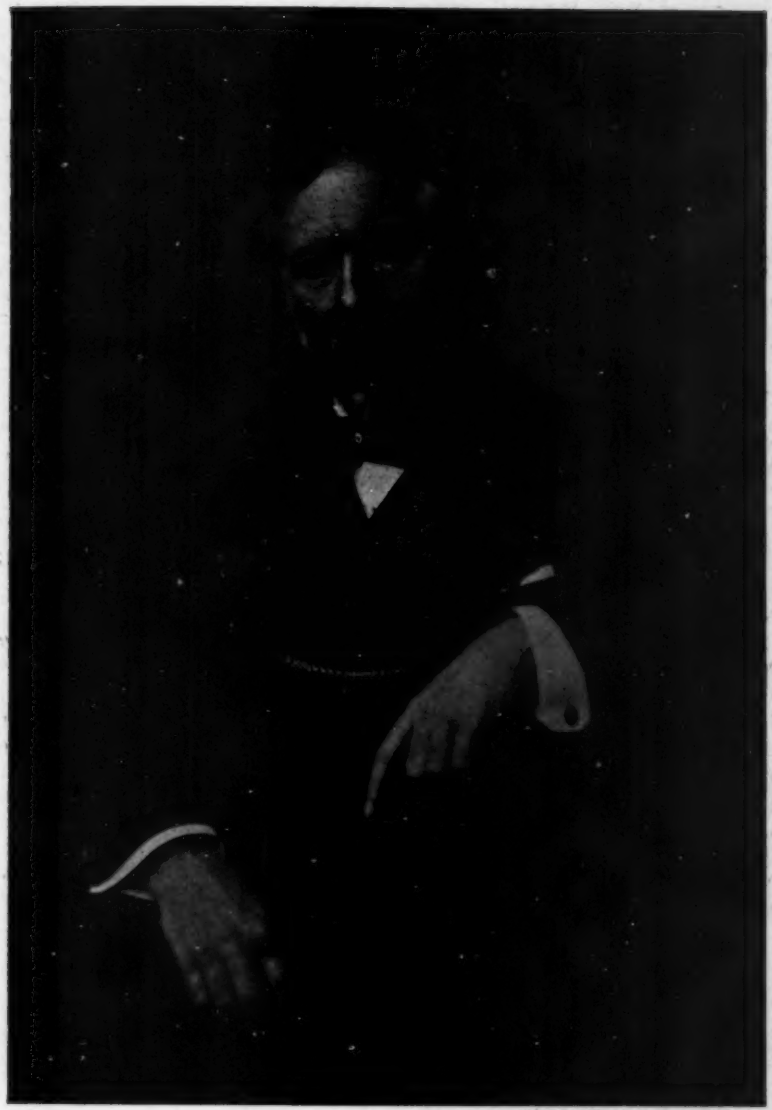

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Adam Smith Professor of Political Economy, University of Glasgow. President of the Economic Science and Statistics Section.

on influence of examinations on teaching, and on the course of experimental, observational and practical studies most suitable for elementary schools, and one on conditions of health in schools.

A special session of the anthropological section was devoted to the question of an anthropometric survey of Great Britain and the alleged deterioration of the people. Mr. J. Gray outlined a plan that had been presented to the Privy Council committee according to which the United Kingdom would be divided into 400 districts, in each of which a representative sample of about 1,000 adults of each sex would be subjected to a large number of measurements and observations. The whole of the school children would be measured, because a thousand of each sex for each age interval of one year would be required, which would amount to about the whole of the school population. The survey would be completed once every ten years, and the total number measured in that time would be about 800,000 adults and 8,000,000 children. It is much to be hoped that the work of the Bureau of American Ethnology may be enlarged so that a similar survey may be made in the United States. Professor D. J. Cunningham and Dr. F. C. Shrubsall read papers, the latter discussing the relations of the blond and brunette types and the fact that the former tends to disappear in cities. Mr. Balfour, who presided over the meeting, remarked that the progeny of every man who won his way from the lowest ranks into the middle class was likely to diminish because of later marriages in that class. Hence it seemed that, as the state so contrived education as to allow this 'rising' from a lower to upper class, by so much did it do something to diminish the actual quality of the breed. It was, of course, not an argument against the state's attitude towards education in this respect; but there was, or seemed to be, no escape from the rather melancholy conclusion that everything done towards opening up careers to those of the lower classes did something towards the deterioration of the race.

In a survey such as this, it has of course only been possible to select from the programs and printed abstracts a few topics from the large number which came before the sections. They indicate, however, the general subjects now engaging the attention of scientific men and call to mind the names of a few of the leaders of science of Great Britain. It seems that on the whole the more eminent British men of science are more likely to attend the meeting of their general association and to take a part in its proceedings than is the case in this country, and it appears also that Great Britain has, at least in the physical sciences, more eminent men than we have.

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UNIV. OF MICH.
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VOL. LXV. No. 6 OCTOBER, 1904

THE POPULAR SCIENCE MONTHLY.

EDITED BY J. McKEEN CATTELL

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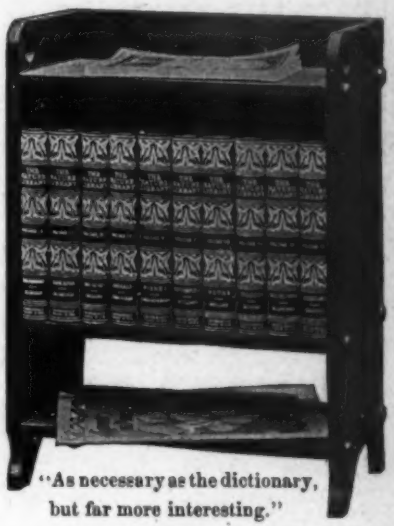

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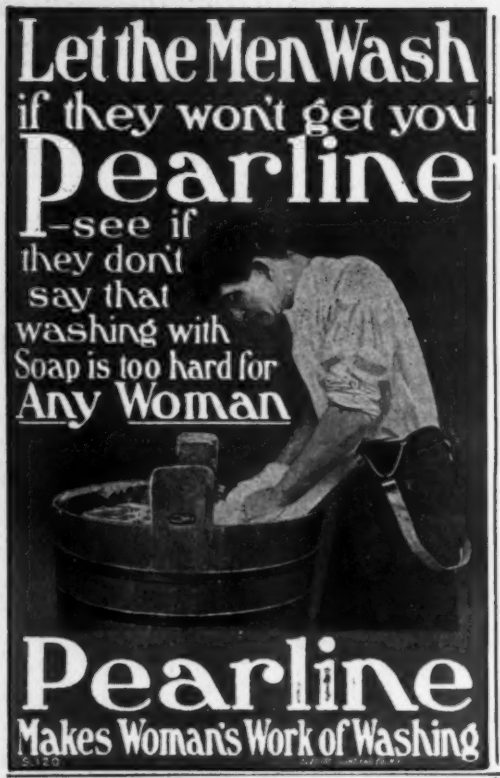

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